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THE MICROSCOPE-CENTRIFUGE AND SOME OF ITS APPLICATIONS.*

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It is a common observation that clay or mud mixed with water gives a turbid suspension, which will finally settle under the influence of gravity. Larger particles separate first, followed by the medium sized particles and, finally, the very finest particles settle out. If the particles are of the same density, a gradation of material appears that is connected with the size of the suspended particles; if particles are of the same size but of different density, they settle out according to their relative density.

Sedimentation by gravity may take an unduly long time, and it is imperative to in some way increase the effect of gravity. Centrifugal force presents a method for doing this. It is hardly necessary to emphasize the importance of centrifugal force in scientific study. By means of the centrifuge forces can be obtained similar to gravity, which otherwise exist only on the very largest stars and could not be attained upon the earth. The attraction on the surface of the sun is something like twenty-seven times that on the earth, while the attraction on the moon is one-sixth of that on the earth; the attraction on some of the asteroids, Eros for example, is

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one one one-thousandth of that on the earth, whereas the attraction on a sun, such as Sirius B, is of the order of twenty-six thousand times that of gravity. By rotating a solution, or anything, fast enough, forces can be attained comparable to those on Sirius and on other stars.

The very simplest kind of a centrifuge is a test tube tied to a piece of string and whirled around the head. It is surprising to calculate what forces can be attained with this simple device. Hand-centrifuges with high gear ratios for throwing out precipitates in urinalysis are found in every laboratory, while double bearing electrical motors are made which will give direct rotations of 10,000 to 18,000 revolutions a minute. Sharples super centrifuges with a single bearing revolve 40,000 R. P. M. and exert forces 41,000 times gravity, while the Svedberg oil turbine develops forces of the order of 100,000 times gravity. Finally, there have been constructed air turbines, air spun tops without bearings, which give rotations of several thousand revolutions a second, and forces approaching 10^6 times gravity. Since centrifugal force is directly proportional to the radius of the centrifuge and to the square of the number of revolutions a second, it is advantageous to use a very high number of revolutions, even though the radius is relatively small.

Since minute particles can be separated from a fluid under proper conditions if their density is different from the fluid itself, some molecules in solution in a fluid can be separated if the centrifugal force is made high enough. Interesting work in this direction has been carried out by Svedberg [1] and his co-workers. Molecules of hemoglobin, the red coloring material in the blood, and other proteins can be separated from solution in a reasonable time. An interesting point observed during the centrifuging process was that the line of demarkation between the hemoglobin and the fluid in which it was dissolved was absolutely clear and distinct, whereas in the sedimentation of a suspension or colloidal solution the line of demarkation is not distinct. Colloidal particles are of different sizes, i.e., there is a distribution of sizes in a colloidal solution, which fall with different velocities, whereas among hemoglobin molecules we have particles of the same size and a clear boundary indicating uniform rate of fall.

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[1] Svedberg, The, Colloid Chemistry, 2d Ed., Chem. Catalog. Co., 1928. Nichols, J. B., *Physics*, 1, 254, 1931.

It is possible, by measuring the rate of fall, to calculate the molecular weight of the hemoglobin molecules. Determinations made by photographing the movement of the boundary agree perfectly with the determinations of molecular weight made by other methods such as osmotic pressure of hemoglobin solutions. It should be pointed out that Svedberg's method of photographing the boundary as it moves past the lens of a camera does not give a true image, but merely records the change in position of a line moving along the axis of rotation, a shadow-graph of the sedimentation recorded from time to time. This is perfectly adequate for the purpose, but not applicable for observation of what goes on in living cells subjected to centrifugal force.

Early use of centrifugal force in biological studies on living cells and organisms involved low rates of rotation, and was designed to analyse the effect of gravity in causing the bending of horizontally placed roots or shoots of plants, positive or negative geotropism. Pflüger [2] (1884) and others used low forces to study the influence on cell division, while Gurwitsch [3] (1904) employed sufficiently high forces to redistribute the suspended materials of eggs and study their influence on subsequent development. Lyon [4] in 1906 first used very high forces (10,000 R. P. M.) which completely separated the oil and yolk granules of marine eggs, forming clearly marked zones of different material, an investigation of fundamental importance, which started a host of sebsequent studies in experimental embryology, which are being continued at the present time.

In general we may say that the centrifuge gives the biologist a means of studying the following problems in cell physiology:

1. Redistribution of visible materials in cells and a study of the effect of this abnormal arrangement on the subsequent history of the cell.

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[2] Pflüger, E., *Arch. f. d. ges. Physiol.*, **31** and **32**, 1883.

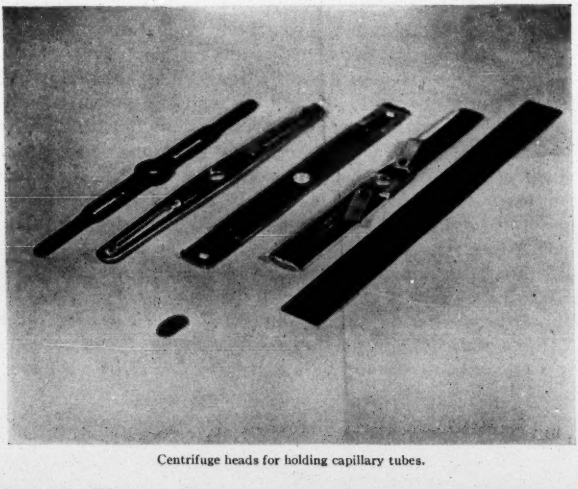
[3] Gurwitsch, A., *Verh. Anat. Ges. Anat. Anz.*, **25**, 1904.

[4] Lyon, E. P., *Amer. J. Physiol.*, **15**, 1906. *Arch. Entw.*, **23**, 151, 1907.

2. Determination of the total volume of visible materials and their relative densities.
3. Determination of the viscosity of the protoplasm of cells and the consistency of visible structures within the cell.
4. Determination of the forces within or at the surface of a cell which counteract the deformation of cells by centrifugal force. This field will be discussed in more detail below.

Previous studies on the effect of centrifugal force on cells have been carried out by placing the material in capillary tubes which are then centrifuged, the centrifuge stopped, cells removed with a pipette and examined under a microscope. Some types of heads for holding capillary tubes are shown in Fig. 1. The end result only of centrifuging is observed. In some cells, like *Amoeba*, return of centrifuged material occurs rapidly, and in any case observation of all stages of a process is far more accurate and satisfactory. The

FIG. 1.



Centrifuge heads for holding capillary tubes.

centrifuge-microscope gives us a method of doing this, of obtaining a perfect image of living cells or any material magnified to the *highest powers* while rotating at *any possible speed.*

It is obvious that a number of methods could be applied, were great magnification and a perfect image of particles of the order of one micron diameter not required. Rotating machinery can be seen as if standing still through a reversing prism, properly aligned, which is rotating at one-half the speed of the object observed. Such a prism could be employed for centrifuge observation, but the mechanical difficulties of alignment and rotation seem insuperable. Another method of observing rotating parts is the stroboscopic one. A microscope, mounted above the rotating material illuminated for a short fraction of a second with each revolution should give a stationary image like a moving picture. It has proved impossible properly to time a spark discharge or lamp for *direct* microscopic observation by this method. Slight irregularities in the moment of discharge are magnified and the discharge itself, even though lasting one-millionth of a second is not rapid enough to give a clear image at high speeds.

However, if the image is reflected to the axis of rotation, irregularities in the time of discharge and the blurring of granules, due to the high peripheral velocity, are completely eliminated. A considerable movement at the periphery becomes reduced at the center in the ratio of the radius of rotation to the radius of observation on the axis of the centrifuge.

The following scheme gives an image that is as perfect as that observed in a stationary microscope with the highest magnification obtainable with dry objectives.[5] The microscope-centrifuge, therefore, is simply a special head containing part of the optical system of a microscope built in place, and revolved by any rotating device. The heads are shown in Fig. 2 and the optical system in Fig. 3. A microscope-objective (obj.) is firmly set into one end of a bar of metal which revolves on the centrifuge axis. The objective is purposely

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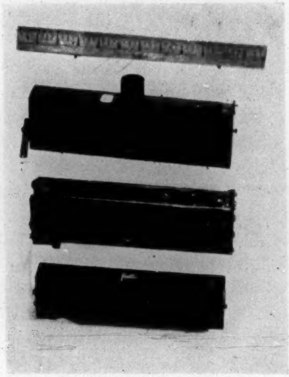
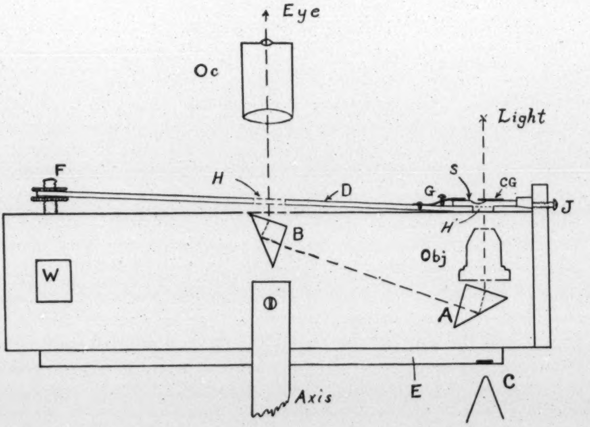
[5] Harvey, E. N., and Loomis, A. L., *Science*, 72, 42, 1930. The centrifuge-microscope heads were constructed in collaboration with Mr. A. L. Loomis at his private laboratory in Tuxedo Park, New York.

FIG. 2.



Centrifuge-microscope heads of three different types.

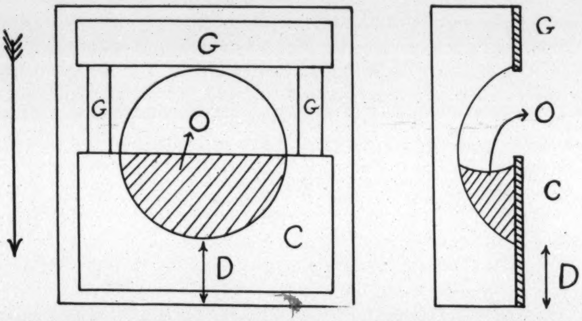
FIG. 3.



Optical system of microscope-centrifuge. *Oc.* ocular; *C.G.* cover-glass; *S*, slide; *G*, spring clip for holding slide; *J*, screw for adjusting slide; *Obj.* objective; *H*, H^1, holes in focusing bar; *C*, contact point; *A*, *B*, right angle prisms; *D*, focusing bar; *F*, focusing screw; *W*, counterweight.

reversed from the ordinary position on a microscope so that the light (*L*), the focusing screw (*F*), and the slide containing living cells (*S*) will be easily accessible on the top of the centrifuge. After passing the objective, the light is carried by two total reflecting prisms, *A* and *B*, to the axis of the centrifuge and then vertically upward. The eyepiece (*Oc*) is stationary and mounted permanently in the protecting cover above the axis of the centrifuge. The counter weight, (*W*) is used for balancing.

FIG. 4.



Slide and top view of slide for centrifuging cells. *C*, cover-slip; *G*, fragments of cover-slip to make slide level; *O*, fluid containing cells; *D*, distance to field of view under objective. The arrow indicates direction of centrifugal force. The curvature of the depression in slide is exaggerated in the side view.

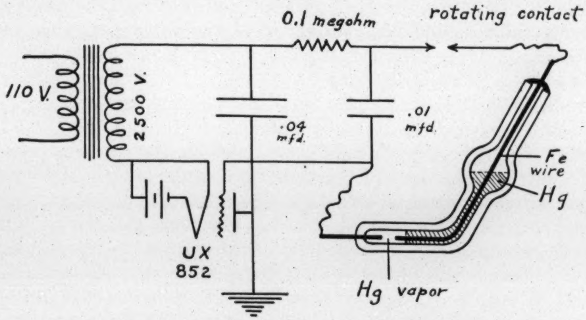
A slide to hold cells (Fig. 4) can easily be made from a hollow depression microscope slide by cementing with picene a coverslip (*C*) over one-half of the depression. There is thus formed a niche (*O*) between cover-slip and surface of slide in which cells are thrown by the centrifugal force. Other bits of cover-slip (*G*) can be cemented around the edge of the depression to prevent evaporation by air currents. For highest speeds the cover-slip must be fused on, forming an all glass slide. If only a few fertilised eggs are placed in the slide there is sufficient oxygen to allow perfectly normal cleavage, and such eggs could be kept under continual centrifuging and observation during development. The slide, cut to proper size, with cover-glass down, is laid on a flat strip of metal

(Fig. 3, *D*) with holes at *H* and *H′* for passage of light, and held down by the clamp *G*. Its position in the direction of the centrifugal force can be adjusted by the screw *J*, so that distance *D* brings cells under the objective. The slide can be focused by moving the whole bar (*D*) up and down on the focus screw *F*, and additional focusing during centrifuging is obtained by changing the vertical position of the ocular. In another design of the microscope-centrifuge head the objective fits accurately in a tube, but can be moved up and down by a rack and pinion (Fig. 2 top). When the slide is in focus a set screw locks the objective in position. This device for focusing does away with the bar *D* and focusing screw *J*, and has worked perfectly at the highest centrifugal speeds.

If the cells are illuminated continuously, the image in the eyepiece while the centrifuge is running, would be whirling in a small circle. However, if we illuminate the cells for a few micro-seconds every time they reach a position in the revolution under the light, the appearance will be that of a succession of images, a moving picture. The cells will appear stationary, the granules within will be seen to move under the centrifugal force.

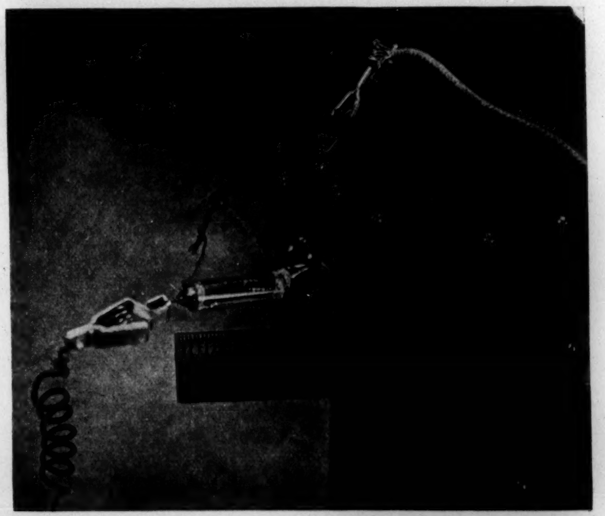
The illumination is a 2,000 to 3,000 volt condenser discharge in mercury vapour at atmospheric pressure or above (Figs. 5 and 6). The lamps can easily be made by sealing a tungsten wire into one end of a pyrex capillary tube which is partly filled with mercury. A portion of the tube near the other end of the capillary is enlarged to a small bulb and an iron wire pushed down the capillary to the proper distance from the tungsten to form the other electrode for the condenser discharge. This iron electrode is then sealed in with de Khotinsky cement, leaving the bulb filled with air. A heating coil around the capillary, not shown in Fig. 5 but apparent in Fig. 6, protected from air currents by an enclosing pyrex tube, boils the mercury in the capillary, forming mercury vapour through which the discharge takes place. A reservoir condenser is kept continually charged to 2,000 to 3,000 volts from a transformer and rectifying vacuum tube. The discharging condenser, of lower capacity, is in parallel with the reservoir condenser through a high resistance, made of a mixture of xylol and methyl alcohol. With each revolution of

FIG. 5.


0.1 megohm
rotating contact
110 V.
2500 V.
.04 mfd.
.01 mfd.
Fe wire
Hg
UX 852
Hg vapor

Lighting system of microscope-centrifuge. The heating coil of nichrome wire around the mercury vapour lamp is not shown.

FIG. 6.



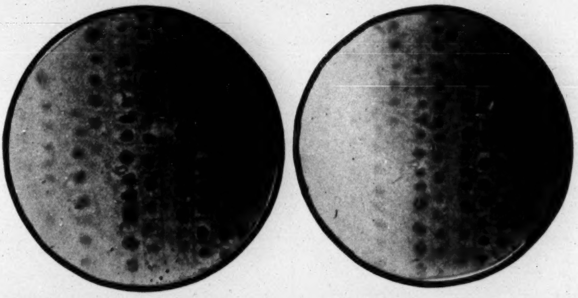
High voltage mercury discharge lamp showing heating coil connections and condenser discharge connections.

the centrifuge the contact point (*C*) on the bakelite disk (*E*), Fig. 3, discharges the 2,000 to 3,000 volts through the mercury lamp, and during the remainder of the revolution the condenser is again charged through the high resistence and is ready for another discharge.

In actual practice the microscope-centrifuge has far exceeded expectations. The image is perfectly clear and steady with the highest centrifugal speed and highest dry objectives (× 62) and, because of the rather long tube length, the magnification obtainable with high power oculars is great. The adjustment of the special slide has given no difficulty. The cells cannot help but be thrown in the proper place for observation once the objective has been focused. Old material can be removed from the slide and new cells inserted in half a minute. In fact a slide of this sort makes a far better container for centrifuging than a capillary tube since the slide can be removed and the centrifuged cells immediately examined *in situ*. A bar of metal with wide grooves at each end and spring clips to hold two of the slides in place in the grooves makes a very convenient centrifuge head for obtaining forces many thousands of times gravity. Fig. 7 shows the type of image that can be obtained with the microscope-centrifuge. The photograph on the right, of a stained section of an onion root tip containing dividing cells showing chromosomes, about one micron in diameter, was taken while revolving 4,000 R. P. M. while the photo on the left was taken while the centrifuge was standing still. The definition of the chromosomes photographed while revolving is quite perfect.

In the original design of the microscope-centrifuge a standard objective and large right angle prisms were mounted on the rotating head. This made the head 2 5/8 inches deep and offered so much air resistance that speeds over 4,000 R. P. M. (about 2,000 times g. at 11 cm. radius) cannot easily be attained. In recent models the air resistance has been greatly diminished by using objective lenses only and small right angle prisms mounted either between objective and object or between the lenses of the objective system. In this way the thickness of the head has been reduced to 1/2 inch, and by stream lining the contours, it is possible to run up to 10,000 (or over) R. P. M. giving forces of 12,000 times gravity. This type of head is most suitable for general biological work, and three examples are shown in Fig. 8. A diagram of the lens system is shown in Fig. 9 and the whole outfit is reproduced in Fig. 10. It will be noted that one form of head (Fig. 8) has an objective and place for a slide at each end, high power to the right and low power to the left. By changing the position of the central prism either power may be used. Fig. 10 shows the motor, steel protecting ring and lighting system, either the mercury spark discharge or an incandescent lamp with a straight coil of incandescent tungsten, in a brass cylinder. A narrow image of the filament is focused on the material by a low power microscope objective.

FIG. 7.



Photographs of stained onion root tip cells showing chromosomes, used as a test object for clarity of image. Photo on right taken while revolving 4000 R. P. M. 5 sec. exposure; on left while standing still.

The periphery of the field of view is blurred by this method of illumination, but the central portion is sufficiently distinct to observe movement of particles one micron in diameter, and the elimination of a rotating contact is highly desirable for high speeds of rotation.

It is possible to go one step further and to adapt the microscope-centrifuge principles to the Beams[6] ultra-centrifuge, by which forces approaching one million times gravity can be attained. This scheme is the simplest possible arrangement and has worked out remarkably well, in fact far beyond

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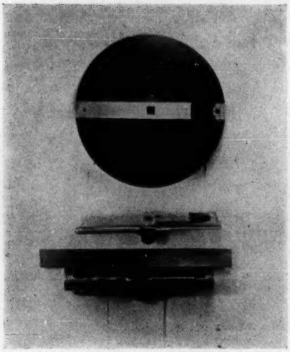
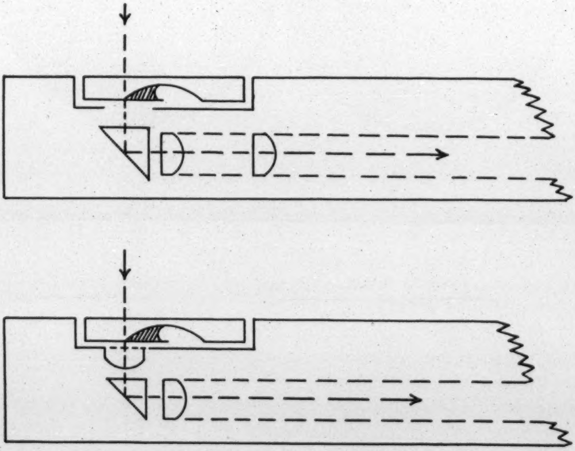
[6] Beams, J. W., *Rev. Sci. Inst.*, 1, 667, 1930; *Science*, **74**, 44, 1931.

FIG. 8.



New types of high speed centrifuge-microscope heads. Top, with bakelite disk contact plate; middle, stream lined; bottom, with high and low power on each end.

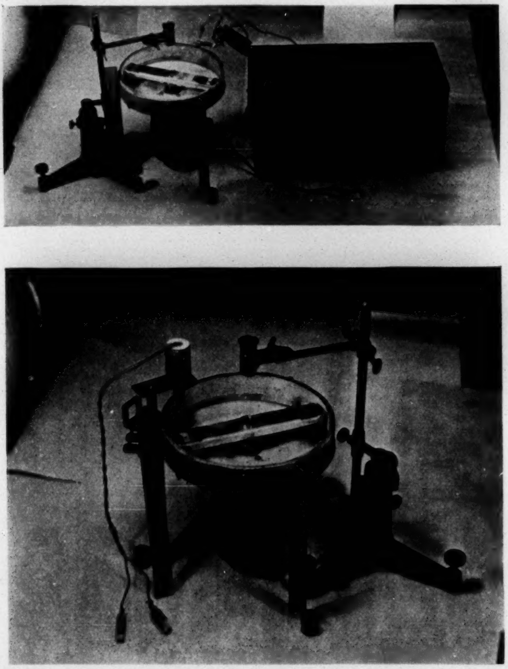
FIG. 9.



Details of optical system of high speed heads for low power (above) and high power (below).

expectation.[7] The Beams super-centrifuge is an air driven spinning top (rotor) revolving on a cushion of air and held in space through the principle of Bernouilli. It is shown in Fig. 11, the middle object resting on a cork, with a cross section in Fig. 12. No lenses but only mirrors revolve. As shown in Fig. 12, two stellite mirrors (M_1 and M_2) are mounted on the

FIG. 10.



Complete centrifuge-microscope for mercury lamp discharge illumination (above) and for incandescent lamp illumination below. The transformers, condensers and resistances for the mercury condenser discharge are in the box.

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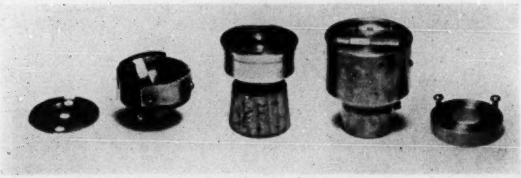
[7] Harvey, E. N., *Science*, 75, 267, 1932.

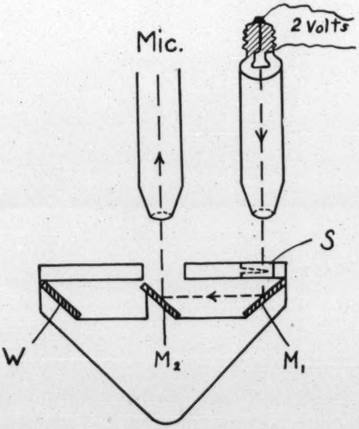
FIG. 11.



Centrifuge-microscope heads for short radius observation. The center cylinder on a cork is the Beams' high speed air driven rotor. To the left is a head for an electric centrifuge with its cover constructed as in Fig. 12, and to the right another type for electric centrifuge with its cover, constructed as in Fig. 18.

Beams rotor in such a position that the image of the object on a special slide (*S*) is brought to the axis and reflected into a complete microscope (*Mic*) mounted above and on the axis of the rotor. *W* is a counter weight for balancing the rotor. The illumination is a narrow image of the filament of a straight filament tungsten lamp thrown on the material to be observed parallel to a radius of rotation. A relatively large movement

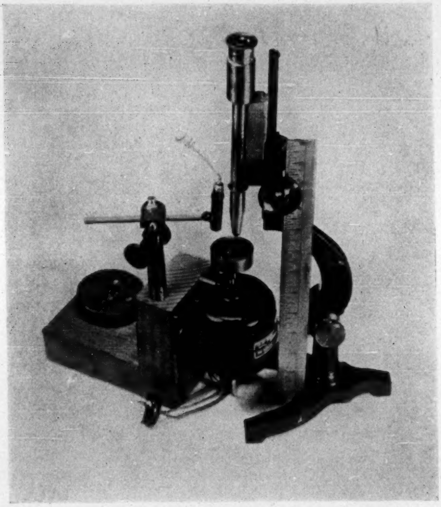
FIG. 12.



Optical system of centrifuge-microscope adapted for Beams rotor. *S*, slide to hold living cells; M_1M_2 stellite mirrors; *W*, counterweight for balancing; *Mic.* compound microscope.

at the circumference becomes a very small movement when the image is brought to and observed on the axis of rotation. While the whole field of view is not perfect, the center is good enough for all practical purposes. The magnification of this scheme is limited by the working distance of the objectives; × 5 to × 7 objectives can be used giving with × 20 oculars, 100 to 140 diameters. By mounting lenses on the rotor higher magnifications can, of course, be used. The centrifugal force attainable by means of the Beam's rotor is limited only by strength of materials, and for microscopic observations by this method, is determined by the strength of the glass container of the living cells. This might be put at 200,000 times gravity. Such an arrangement should be particularly useful for determining molecular weights of substances by the method of sedimentation, for observing movement of mate-

FIG. 13.



Short radius centrifuge-microscope complete for electric motor drive.

rials in highly viscous or gel containing cells and for observing the change in shape of living cells due to the stretching forces of light and heavier material. In order that a cell may not be completely crushed by forces thousands of times gravity, it is necessary to adopt the expedient of suspending the material in a medium of graded density, so that the cell comes to lie in a stratum of equal density, and is thereby perfectly cushioned against crushing. Figure 13 shows this type of centrifuge-microscope adapted for low power observation of organisms rotated by an electric motor and Fig. 11 (left) shows an enlarged view of the head.

In determining the viscosity of cells by the centrifugal method application is made of Stokes' law for the fall of a particle in a fluid:

$$V = \frac{2cg(\rho_1 - \rho_2)r^2}{9\eta},$$

in which V is velocity of fall; c, centrifugal force in times gravity; g, force of gravity, 980; ρ_1 and ρ_2, density of particle and medium; r, radius of particle; η, viscosity of medium. A correction must be made, the Cunningham correction, to allow for the influence of many particles falling together, if absolute viscosities are to be determined. Work on the viscosity of protoplasm has been largely carried out in this country by Heilbrunn.[8] The experiment is performed by centrifuging cells in capillary tubes for various times and with various forces removing them from the tubes and examining under an ordinary microscope, to find how long it takes to move the granules a certain distance. With the centrifuge-microscope this movement can be watched as it takes place. By means of an ocular micrometer the velocity of individual granules can be measured directly or cells in two different conditions can be observed simultaneously, and very exact measurements thereby made. One interesting thing that has appeared in the direct observation of cells is the non-uniform fall of small particles. They move in jerks as if they fell through a meshwork occasionally striking on a mesh, or as if they moved along a jelled surface to which they stuck

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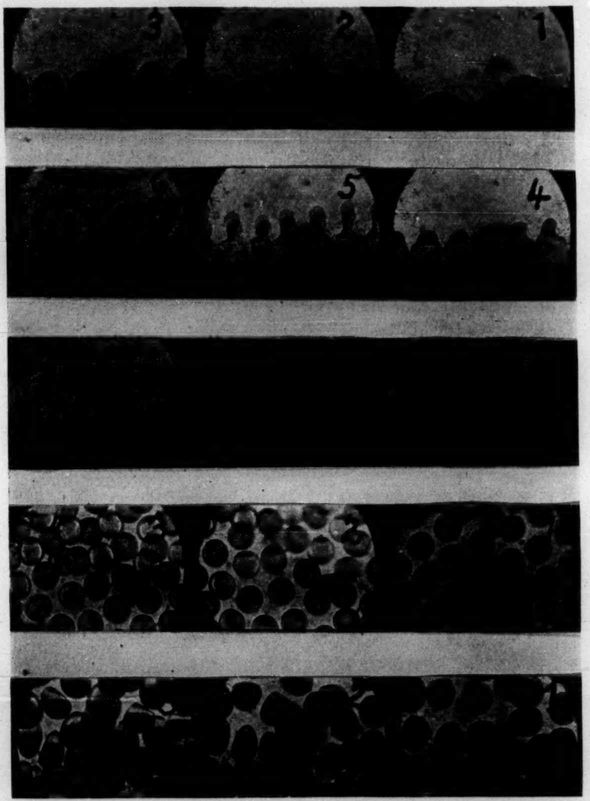
[8] Heilbrunn, L. V., The Colloid Chemistry of Protoplasm. Geb. Borntraeger, Berlin 1928.

FIG. 14.



Photographs of unfertilized eggs taken with a Leica camera through the microscope-centrifuge (radius 11 cm.), showing successive changes (examine from right to left) in stratification of granules and in shape of eggs. Magnification, 87 diameters. The direction of the centrifugal force is downward.

First three rows, *Chætopterus* exposed for 5 seconds at 35, 55, 95, 165, 205 and 265 (double exposure), 345, 375, 450 and 480 seconds after starting centrifuge. Speed of the microscope-centrifuge was 55 R. P. S. in the first row and 66 R. P. S. in the second and third rows. At the highest speed about 330 flashes of light occurred during the exposure.

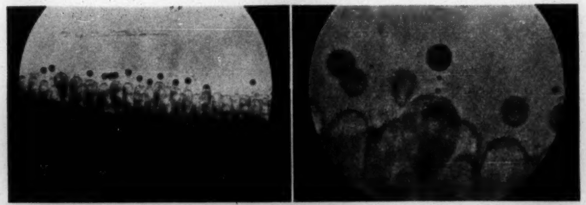
Fourth row, *Cumingia* exposed 10 seconds at 35, 95 and 155 seconds after starting centrifuge. Speed 38 to 55 R. P. S. The eggs are separated by jelly.

Fifth row, *Arbacia* exposed 10 seconds at 45, 150 and 285 seconds after starting centrifuge. Speed 35 R. P. S. The eggs are partially separated by jelly.

from time to time.[9] This behavior along with the Cunningham correction and the minute size of the particles observed, whose radius (squared in the Stokes' equation) cannot be measured with high accuracy, makes the calculation of absolute viscosity somewhat doubtful, but for relative viscosities the centrifuge method has yielded results of very great interest.

One of the most interesting applications of the microscope-centrifuge is to study the deformation of cell parts and particularly of cells under the influence of centrifugal force. Most cells contain oil globules which are lighter than water and yolk granules which are heavier than water. The two are separated by centrifuging and exert stretching forces which divide the egg into fragments or two halves. Fig. 14 shows the effect in the case of eggs of a marine worm, *Chætopterus*, where successive photographs taken through the microscope-centrifuge about one-half minute apart show the pulling off of spherules of oil. If the centrifuge is stopped the oil spherules tend to round up and present the appearance of Fig. 15, stationary photographs taken under low power (left) and high power (right) objectives. Other eggs pull into two nearly equal halves. Figure 16 is a photograph of three stages of this process in the egg of *Arbacia*, showing the relative volumes occupied by different zones of granules. We may regard the spherules of oil as in part pulling against the tension of the egg surface and in part overcoming viscous forces within

FIG. 15.



Chætopterus eggs photographed after centrifuging to show rounding up of oil spherules and protoplasm. Low magnification on left and high on right.

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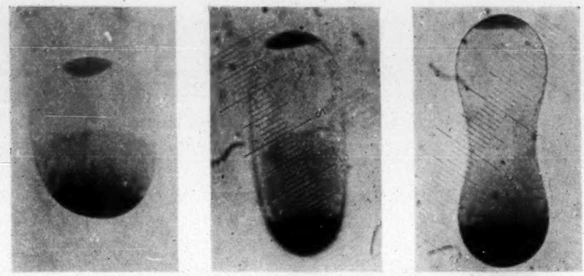
[9] Harvey, E. N., *Journ. Exp. Biol.*, 8, 267, 1931.

the egg. In the case of *Chætopterus*, by measuring the volume of the oil spherule, assuming a reasonable value for its density and finding the centrifugal force necessary to pull off an oil spherule, the tension around the circumference of the stalk can be calculated. It turns out to have a *maximum* value of less than one dyne per cm., much less than had previously been supposed, and something like 0.01 of the surface tension of water-air.[10]

FIG. 16.



Arbacia eggs showing stages of pulling into two halves on centrifuging by stretching forces of light oil (top) and heavy yolk. From measurement of the centrifugal force necessary to do this calculation of the tension at the surface of the egg may be made. The uncentrifuged egg is spherical with even distribution of oil and yolk.

In the case of the sea urchin, *Arbacia*, centrifugal force is able to pull the egg into two almost equal halves (Fig. 16). We may regard the egg as a sphere stretched to a cylinder which becomes unstable like any cylinder when its length is about three times its diameter. The tension around its circumference then balances the stretching forces of the heavy yolk (V_H) and light oil half (V_L). It is easy to calculate that the radius, r, of a cylinder whose height is π times its diameter and of the same volume of an *Arbacia* egg (whose diameter is 74 micra) will be 23 micra. Hence $2\pi rT$ = forces stretching cylinder, when T = tension of the surface. If the egg is suspended in a medium of the same density (ρ_M) the stretching forces are $[V_H(\rho_H - \rho_M) + V_L(\rho_M - \rho_L)]\ 980 \times C$ where ρ_H = density of heavy half egg, ρ_L = density of light half egg and C = centrifugal force in terms of gravity.

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[10] Harvey, E. N., *Biol. Bull.*, **60**, 67, 1931.

Since the egg pulls into approximately equal parts, the density of the light fragment must be as much less than the medium as the density of the heavy fragment is greater than the medium. If V_E is the volume of the egg, the whole relation therefore becomes:

$$2\pi r T = V_E(\rho_M - \rho_L)980 \times C.$$

It is only necessary to know the diameter of an egg, the density of the medium and that of the light fragment and the centrifugal force to divide the eggs.

Values obtained by this method are about 0.2 dyne per cm. an extraordinarily low tension.[11] It should be pointed out that the egg surface is treated as if it possessed a true surface tension. There is undoubtedly a very thin membrane present at the surface which behaves like a surface. Cole [12] has shown by an extremely ingenious and delicate spring "egg crusher" by which the force necessary to deform a single egg can be measured, that the surface is elastic and requires greater forces to deform it when stretched. Cole's values by a totally different method of measurement agree perfectly with those obtained by the centrifugal method.

This method can also be used to study distortion of *Paramœcium* (Fig. 17) or the force necessary to pull oil out of *Amœba*. Work along these lines is now in progress and it rather looks as if it were necessary for many cells to have a very low tension around the surface, i.e. to make themselves independent of even so small an interfacial force as that between air and water, 76 dynes per cm.

An interesting application of the centrifuge has been made to study the force exerted by a small unicellular organism, *Paramœcium*, in swimming by means of its cilia. The animal is negatively geotropic and swims against gravity. When centrifuged a force can be found against which it just fails to swim. By knowing also its volume and the difference in density between *Paramœcium* and medium Jensen [13] found that one *Paramœcium* .25 mm. long and weight .000175 mg. could lift .00158 mg., or 9 times its own weight. 600 *Para-*

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[11] Harvey, E. N., *Biol. Bull.*, **61**, 273, 1931.

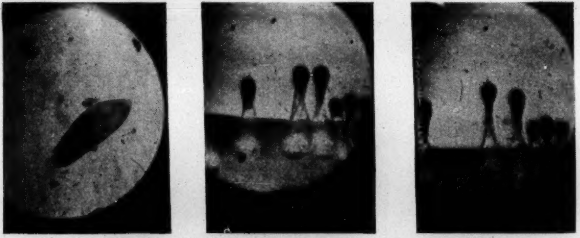
[12] Cole, K. S., *J. Cell. and Comp. Physiol.*, **1**, 1, 1932.

[13] Jensen, P., *Arch. f. d. ges. Physiol.*, **54**, 537, 1893.

mœcia could lift a milligram. Jensen's original experiments were carried out by centrifuging in tubes, stopping the centrifuge and quickly examining the position of the *Paramœcia* in the tubes. There are two methods by which small organisms can be observed under low power while responding to low centrifugal forces, illustrated by Figs. 18 and 19.

The organisms are mounted on a hollow depression (*D*) slide (*S*) over which a glass cover (*C*) is placed (Fig. 18). This mount rests on a disk (*A*) having a groove in which a piece of metal (*B*) slides, its position adjustable by screws (*EE*). A right angle prism (*P*) is mounted in *B*. The whole is held in position by a ring *R* (and screws (*FF*)), which fits the rotary container screwing on the shaft of a centrifuge motor. From the light *L* the image of a straight filament incandescent lamp is focussed on the edge of the depression, parallel to a radius of rotation. The reflected image of organisms at that point is observed on the axis of rotation with a microscope, *M*.

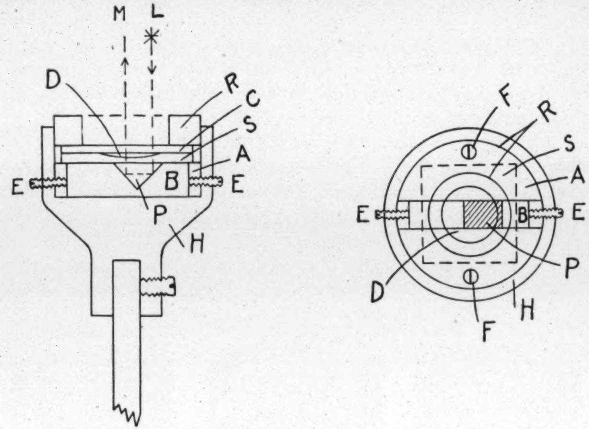
FIG. 17.



Photos of Paramœcium deformed by centrifugal force. Left—stationary; middle—10,000 R. P. M. one minute; right—10,000 R. P. M. five minutes, ten seconds exposure.

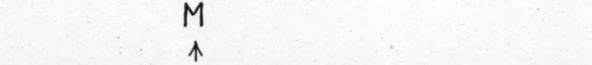
Another method (Fig. 19) of observing animals while revolved on a short radius is to mount the organisms on a depression slide, *S*, as before covered with a cover-slip, *C*, and held in place by the clips *K*, *K*. A prism, *P*, reflects light from a high voltage mercury discharge lamp, *Hg*, through the organisms, which are observed with a microscope at *M*. A contact point (*Con.*) on an insulating disk discharges the condensers, *mf*, through the mercury lamp with each revolution. This device gives a perfect image over the whole field of view.

FIG. 18.


M
L
D
R
C
S
A
B
E
E
P
H
F
R
S
A
E
B
E
P
D
F
H

Side and top view of short radius centrifuge-microscope head shown in Fig. 11, right. Explanation in text.

FIG. 19.


M
K
D
C
S
K
L
Hg
E
Con.
P
.1 mf.
.01 mf.
0.1 meg. Ω
2000 V

Side view of another type of short focus centrifuge-microscope for observing the behavior of organisms under low centrifugal forces. Explanation in text.

There are many other uses to which a microscope-centrifuge may be put, for example a study of stresses in crystals and transparent substances by use of polarized light. Observation of any small object while whirling at the greatest possible speeds is so easily carried out that the centrifuge-microscope should be a most useful instrument in every laboratory.

Cellulose Acetate Plasticized. (*Chem. & Ind.*, *51*, 371.) Most of us know that cellulose acetate silk is the youngest member of the artificial silk family. A chemical combination of cellulose and acetyl, its properties differ from those of viscose rayon to the extent that a wholly new variety of dyes had to be developed in order that it might be colored as brightly as the older members of the family. Unlike nitrocellulose, cellulose acetate could not be kneaded into a plastic, doughy mass with camphor and alcohol to give a celluloid. The presence of camphor is necessary in the celluloid, for without it the nitrocellulose would set up to give a hard, brittle mass when the alcohol had become evaporated. The camphor serves to keep the celluloid flexible and is known as a plasticizer.

It so happens that camphor cannot be used as a plasticizer for cellulose acetate; in fact, no one substance is known at this time that will accomplish it alone. It is necessary to use a mixture of plasticizers the ingredients of which depend on the method of processing, the nature of the cellulose acetate used, and the properties which is desired to impart to the product. This plasticized cellulose acetate may be formed into films, foil, coated wire netting, sheets, rods, tubes or hard slabs. It is a true thermoplastic in that it can be shaped or moulded under heat and pressure and will retain the new shape permanently at ordinary temperatures.

A particular variety of this material is sold under the trade name: "Lamarith." Lack of brittleness is one of its outstanding characteristics. It has a dielectric strength of 400 to 600 volts per mil. It can be ignited by holding in a flame but it will not support combustion after the flame is removed. It can be made crystal clear and a sheet 0.0035 in. thick will transmit 80 per cent. of the ultra-violet light in the sun's rays. It can be made into clear or colored transparent foils which are so popular as a wrapping material. It is unique in that the laps can be sealed to give a joint as strong as the rest of the foil merely bv moistening it with a cellulose acetate solvent.

T. K. C.

TRANSMISSION OF ELECTRONS BY THIN FILMS; AN HISTORICAL RÉSUMÉ.

BY

MILDRED ALLEN, Ph.D.

BARTOL RESEARCH FOUNDATION
Communication No. 66.

When an electron passes through a thin film, there are three things which may happen to it: (*a*) It may suffer a change in direction without change in velocity, owing to a collision with a nucleus. (*b*) It may suffer changes in velocity and in direction owing to collisions with other electrons; these may be large or small. (*c*) It may knock an electron out of an atom, thus ionizing that atom. To these three effects many now be added the diffraction effects without velocity change arising very vividly from its wave character as illustrated in the experiments of Davisson and Germer,[1] G. P. Thomson,[2] Rupp[3] and others, and depending upon the crystalline arrangement of the atoms; these, however, will not be discussed here. The theoretical explanation of these various effects is not to be attempted in this paper. It will concern itself with the changes in velocity suffered by an electron in passing through a thin film, the variation in absorption of these electrons with the velocity of the beam and the thickness of the film, and the angular distribution of the transmitted electrons. When in connection with any of these problems an author has attempted to find evidence for the ionization of the atoms of the film, inasmuch as ionization by electrons has been very little investigated except for the case of gases, this will be pointed out together with whatever reasons are obvious for his failure to detect such evidence. It will appear that, in spite of the enormous amount of experimental work which has been done on the transmission of electrons by thin films, the whole story has by no means been told with respect to such transmission.

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[1] Davisson and Germer: *Phys. Rev.*, **30**, 705, 1927.

[2] Thomson, G. P.: *Proc. Roy. Soc.*, **117**, 600, 1928.

[3] Rupp: *Ann. d. Physik*, **85**, 981, 1928.

Early History. The earliest work on the passage of electrons through thin films was done in 1894 by Lenard.[4] As a result of his work aluminium windows came into vogue for the transmission of cathode rays, since thin films of aluminium transmit a very large percentage of the impinging electrons. For instance, forty-five per cent. of 24000 volt electrons would be transmitted by a window 10^{-4} cm. thick.[5] Lenard, and his student and colleague, Becker, have continued the work on transmission of electrons up to the present and have just published in the Handbuch der Experimentalphysik [6] a detailed account of the history and present status of electron transmission.

In the earlier experiments, many of them performed with radium as a source of beta-rays, it seemed that the rays suffered no change in velocity in passing through the thin film and that the absorption of the electrons was proportional to the mass per unit area of the film independent of the substance used. With more accurate experiments both these conclusions have later been shown to be only approximately true. As regards the loss of velocity, W. Wilson [7] showed in 1910 that there was a real, although small, loss in velocity as electrons passed through thin films of aluminium. Crowther [8] at about the same time and Leithauser [9] somewhat earlier also gave evidence for this loss of velocity. Wilson's method was to use radium emanation as a source of high speed electrons. He sorted out a beam of these of uniform velocity by deflecting them magnetically through 90°, passed this uniform beam through the film, and analyzed the resulting beam by again deflecting it magnetically through 90° into the ionization chamber. As he added films, the maximum, besides becoming lower, moved towards lower velocities. Whiddington [10] in 1912, again by a double magnetic analysis, not only showed this change in velocity qualitatively, but showed quantitatively that the difference in the fourth powers of the velocities of the incident and emergent electrons was proportional to the thickness of the film, as required theoretically in a law proposed by J. J. Thomson.[11] The constant of proportionality was found numerically for aluminium, gold and air. More recently Terrill,[12] with the modern technique of the hot filament tube, has repeated such measurements for several elements and determined the constants of proportionality for them. It is interesting to note that in making his magnetic analysis he deflects his electron beam through an angle of only 30°, and thus does not take advantage of the increased resolving power and focussing possibilities offered by the semi-circular focussing method. In the last five years, however, perhaps the most interesting work in this line has been done by Becker, Klemperer, Schonland, and White and Millington, and their work will now be considered individually and in greater detail.

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[4] Lenard: *Wied. Ann.*, **51**, 225, 1894.

[5] *Handbuch der Physik*, XXIV, p. 34.

[6] Lenard and Becker: *Handbuch der Experimentalphysik*, vol. XIV.

[7] Wilson, W.: *Proc. Roy. Soc.*, **84**, 141, 1910.

[8] Crowther: *Proc. Roy. Soc.*, **84**, 226, 1910.

[9] Leithauser, E.: *Ann. d. Physik*, **15**, 283, 1904.

[10] Whiddington, R.: *Proc. Roy. Soc.*, **86**, 360, 1912.

Becker.[13, 14] Becker's earlier paper is a study, for cathode rays of about half the velocity of light, of their loss of velocity and absorption in aluminium films. To excite the cathode rays he uses a gas tube, instead of a tube of the new hot filament type, with an induction coil for his source of potential. Such an arrangement is not usually conceded to give a very steady or uniform beam, but Becker, by keeping the pressure in the tube very constant, succeeds in getting a perfectly satisfactory beam of electrons, uniform in velocity and intensity. In the photographs reproduced in his paper, both the direct and deviated beams are very sharp, thus giving evidence of this uniformity. The rays pass through a thin aluminium window dividing the excitation chamber from the observation chamber and then through two narrow slits, 0.11 mm. and 0.05 mm. in width, across one of which is placed the film to be studied. A photographic plate 3 cm. long is placed 6 cm. behind these slits. A known magnetic field between the slits and the plate may be applied perpendicular to the direction of motion of the electrons so that the deviated as well as the direct beams may be recorded on the plate. With a given magnetic field these deviations are found with and without the film, and by comparing the deviations in the two cases the loss of velocity in the film can be computed. The deflected beams, without passing through a film, show a slight haziness on the low velocity side, but with the insertion of the thin film, the lines are broadened and become distinctly more hazy on the low velocity side, as was clearly shown when the lines were studied with a microphotometer. Since much longer exposures are required for these transmitted lines than for the blank runs, the transmitted lines are known to be much weaker. Thus these experiments show three things, that the electrons are partially absorbed in passing through the film, that they suffer a slight change in velocity and that their velocity is less uniform after transmission than before.

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[11] Thomson, J. J.: *Conduction through Gases*, p. 378, 2nd edition, 1906.

[12] Terrill: *Phys. Rev.*, **22**, 101, 1923.

[13] Becker, A.: *Ann. d. Physik*, **78**, 209, 1925.

[14] Becker, A.: *Ann. d. Physik*, **84**, 779, 1927.

Becker next studies the absorption of cathode rays as a function of the thickness of the absorbing film. Since he takes no precautions with regard to the secondary and reflected electrons, his results must give only apparent absorptions, as we shall later see the term defined by Schonland. Increasing absorption with decreasing velocity is clearly shown, and the absorption as a function of the mass per unit area is found to be very nearly the same for aluminium and platinum. The velocity distribution of the transmitted electrons is here unknown, since they are not analyzed by a magnetic field, but are all received by a Faraday cylinder. He also makes some attempt at a direct determination of the range of electrons in aluminium and as far as it goes this seems to agree with the rest of the work in the paper.

Becker makes no mention of trying to find evidence for the ionization of the atoms in the transmitting film. However, the method would not seem to lend itself to such an end, for because of the comparatively few electrons which succeed in knocking out a K-electron, the intensity of the line corresponding to the electrons having lost this velocity would probably be indistinguishable from the fogged background which he says results from the excitation of the gas molecules remaining in the vacuum.

In his later paper Becker works with slower electrons, below 4500 volts velocity, and investigates the transmission of nickel, aluminium and gold films. In this apparatus he has what may be called a double Faraday cylinder, with the film interposed between the two parts and attached to the first part, which arrangement enables him to measure separately the incident and transmitted beams. In determining the transmission coefficients he makes corrections for the secondary electrons and for the lack of homogeneity of the primary beam, but makes no correction for the electrons reflected back from the incident side of the film. He deals with the velocity distribution of the emergent electrons by the method of retarding potentials. It is interesting to note, that in spite of his prejudice for gas tubes, he here works with one of the hot filament type. Nickel films, 4×10^{-6} cm. thick, were carefully studied as regards their electron transmission throughout the entire range of velocities. The results using the different films agree well with one another. The transmission coefficient seems to vary smoothly with the velocity, and this eliminates the possibility of anomalous transmission which might be expected in the neighborhood of 90 volts where Davisson and Germer found anomalous dispersion. The absorption, of course, increased very rapidly with a decrease in velocity. In a later paper [15] Becker studies further the transmission by nickel films of electrons having velocities between 0.5 and 100 volts. He finds that the electronic transmission of the film increases most rapidly between 2 and 10 volts; evidence for the existence of anomalous transmission is not convincing.

In plotting the number of transmitted electrons against the retarding potential, the curve was very steep at first, owing to the great number of secondary electrons of very low velocity. It then became more nearly horizontal, showing the presence of primary electrons with a continuous distribution of velocities between zero and a certain maximum value, but having lost a considerable velocity. There follows beyond this flat region a more or less sharply defined point of inflection, which denotes a maximum in the velocity distribution at this point. The reason for these points of inflection is vague; they do not correspond to characteristic velocities since they are not fixed in position. They do show some deviated as well as the direct beams may be recorded on the plate. With a given magnetic field these deviations are found with and without the film, and by comparing the deviations in the two cases the loss of velocity in the film can be computed. The deflected beams, without passing through a film, show a slight haziness on the low velocity side, but with the insertion of the thin film, the lines are broadened and become distinctly more hazy on the low velocity side, as was clearly shown when the lines were studied with a microphotometer. Since much longer exposures are required for these transmitted lines than for the blank runs, the transmitted lines are known to be much weaker. Thus these experiments show three things, that the electrons are partially absorbed in passing through the film, that they suffer a slight change in velocity and that their velocity is less uniform after transmission than before.

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[15] Becker, A.: *Ann. d. Physik*, **2**, 249, 1929.

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[15] Becker, A.: *Ann. d. Physik*, **2**, 249, 1929.

Klemperer.[17] Klemperer used a photographic method to detect the electrons which after transmission through a thin film F were analyzed by a magnetic field and focussed on the plate PP by the well-known semi-circular method. Figure 1 shows the general arrangement of this apparatus. Very sharp lines were obtained by using sufficiently narrow slits, S, S, 0.1 and 1.0 mm., and by carefully adjusting the position of the filament with respect to these slits. Holes were punched in the films so that there would be present on the plate the direct beam from which to measure the position of the transmitted beam. The photographs then showed a narrow bright line, a dark space, and then a bright band gradually shading off. The photographic method is open to criticism, as Klemperer himself points out, if in interpreting the photographs one attempts to draw conclusions as regards relative intensities, unless the variation of intensity of the photographic line with the number of electrons, their velocity, etc., is known, and such data were not available at the time of this paper. The position of the maximum is, however, independent of these considerations and hence one may have confidence in the values of the fourth power law constants which he finds from these experiments for electrons with velocities between four and thirteen kilovolts. For aluminium his value of this constant agrees very closely with that found by Whiddington and is about half that found by Terrill; that difference could be explained if the constant does increase with the velocity as has been suspected, since Terrill used much higher voltages. From this work Klemperer concludes that with "thick films and low velocities with a completely homogeneous primary beam the transmitted beam shows a wide distribution of velocities, while if one goes to higher velocities and thinner films one arrives at apparently homogeneous transmitted beams in agreement with Lenard and Becker. Any discontinuity in the curves which might point to different energy levels of the atomic electrons, and to find which was one of the principal objects of this research, was entirely absent. . . . The curves are smooth and show no discontinuities; hence one must assume that the cathode rays very seldom lose energy by knocking an electron out of the inner energy levels." This is not surprising with a photographic method, since Klemperer also reports that long exposures fogged the plates. He used Schumann plates and got very detailed pictures in one minute with a current of

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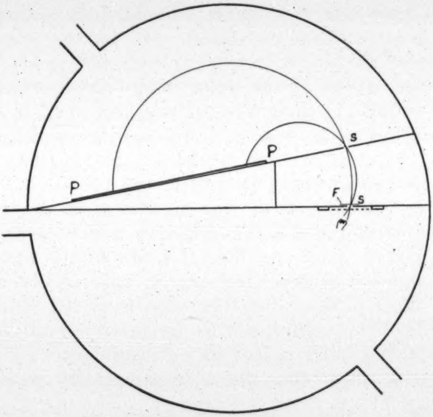
[16] Hartig, H. E.: *Phys. Rev.*, **26**, 221, 1925.

[17] Klemperer: *ZS. f. Physik*, **34**, 532, 1925.

FIG. 1.



10^{-5} amperes. In view of the very low efficiency of x-ray production by electron impact the number of electrons successfully producing x-rays might easily not have been greater than one thousandth of this, and the premature fogging of the plates would prevent the possibility of making long enough exposures to find them if they did exist.

Schonland.[18, 19] Schonland of South Africa in 1923 and 1925 did an ingenious piece of work on the real absorption of thin films for reasonably fast cathode rays. Experimentally Schonland follows Becker in using a gas tube run by an induction coil. This has the advantage over a hot cathode tube that after being opened to put in a new film, if the tube has once been thoroughly outgassed, it pumps down in a short time without re-baking. He follows Whiddington in deflecting the electrons through only 90° to attain homogeneity and works at the point where the intensity of the electron beam, i.e. their number per unit area, is the least. He thus takes no account of the advantage which he might have had by semi-circular focussing. In his firing tube, he uses a divergent beam of electrons.

The ingenious part of this apparatus is the system of measuring cylinders, *A* and *B*, Figure 2, which are so arranged that the electrons reflected back from the film *F*, the absorbed current and the transmitted are all successfully separated and the effect of secondary electrons from the film is eliminated. The thin film itself is mounted on an insulating mica plate, but is connected to a galvanometer by means of fine silver springs; thus the charge actually received and held by the film is measured directly. The film is protected on either side by a grid *G*—0.4 cm. mesh—carrying a retarding potential of 100–200 volts which successfully drives back to the film all secondary electrons since these have been shown by more than one observer to have velocities of not more than 30 volts. These grids seem to introduce no additional complication. On either side of the film are two brass cylinders, *A* and *B*, insulated from each other; that on the incident side accumulates a charge from the reflected electrons, while the other measures the transmitted charge. Even

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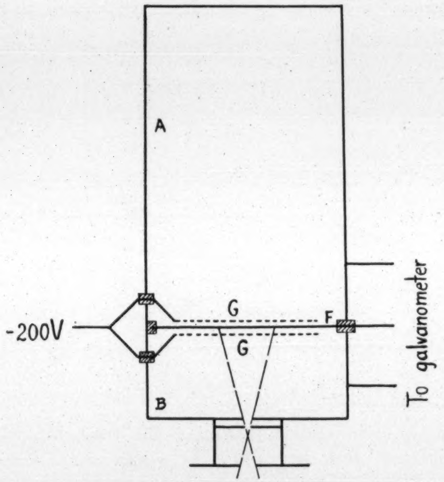
[18] Schonland, B. F. J.: *Proc. Roy. Soc.*, **104**, 235, 1923.

[19] Schonland, B. F. J.: *Proc. Roy. Soc.*, **108**, 187, 1925.

without a film a small reflected charge was found, and this "zero error" was corrected for in interpreting the results.

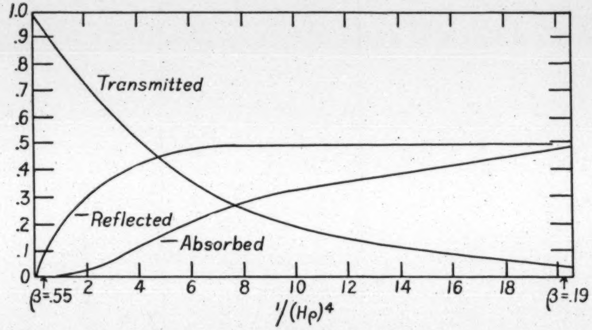
First, a study was made of the effects arising from a variation in the velocity of the cathode rays; and the variation of the fraction reflected, absorbed and transmitted was plotted as a function of the velocity, Figure 3. It is pointed out that most observers have measured the apparent absorption, that is, the fraction not transmitted, whereas in reality this part is only partly absorbed, the rest having suffered reflection from the incident surface. The true absorption part is much the same for all metals and follows an inverse fourth power law, whereas the reflected portion varies markedly for different elements and different speeds.

FIG. 2.



Secondly, a study was made of the effect of varying the thickness of the film. "For thin foils rays pass through with diminished velocity, but very little absorption occurs except for these few rays which are so scattered by close nuclear collisions as to have a much increased length of path in the matter. As the thickness is increased this scattering becomes more and more important, throwing rays out of the main line and increasing their path to a length necessary for complete stoppage. Eventually the actual thickness of the foil is sufficient to stop even those rays which escape close nuclear collisions, and the whole beam is absorbed, with the exception of the portion which is scattered back on the side of incidence." This is clearly shown in Figure 4, where the lower curve gives the variation of the transmitted fraction with thickness and the upper the variation of the fraction truly absorbed. The difference between the two is due to reflected electrons. A recent paper by Eddy,[20] using a radioactive source and a Geiger counter, while interested primarily in the angular distribution of the transmitted electrons, corroborates Schonland's results in that the central part of his curves for aluminium, copper and silver, representing the fraction transmitted as a function of the thickness of the film, is found for all angles to be straight and the initial and final parts flattened out as in Figure 4. By extending the linear portions of these curves to cut their asymptotic values, the practical range of the electrons of the

FIG. 3.



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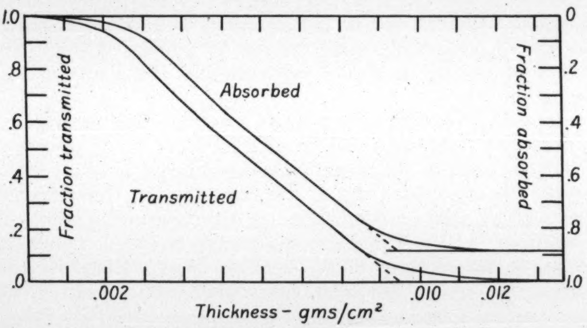
[20] Eddy, C. E.: *Proc. Camb. Phil. Soc.*, **25**, 50, 1929.

given velocity may be determined; the values found from the absorption and transmission curves are seen to agree well with each other. The range found in this way agreed also with the values found experimentally by others and anticipated by Bohr's theory.[21] Bohr's theory is based on the assumption that the loss of energy of the moving particles is to be ascribed to a gradual loss of energy as they encounter the electrons of the atoms they penetrate. Schonland's work then supports this theory rather than the opposite one advanced by Lenard that a cathode ray may be stopped in mid-career and lose its kinetic energy in a single encounter with some center of force in the atom.

FIG. 4.



At the very end of the paper Schonland considers the possible effect of crossing a characteristic x-ray level, and decides that on any theory the effect to be expected would be very small. For gold, making an experimental test of the question, he finds no change in the slope of the curve at the critical *L* potential. However, there is reason to believe that the number of electrons experiencing this phenomenon would be sufficiently small to render them undetectable with the degree of sensitivity employed in these experiments.

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[21] Bohr, N.: *Phil. Mag.*, 30, 581, 1915.

White and Millington.[22] The very recent piece of work by White and Millington is devoted to the study of the loss of velocity of high speed electrons from a radioactive source in passing through thin films of mica. The method was that of magnetic analysis with semi-circular focussing, the detector being a photographic plate. The variation in intensity of the electron beam is determined by a microphotometric study of the photographic image, a method which is no longer open to criticism inasmuch as the photographic action of cathode rays has recently been investigated. The specific aim is to determine the velocity distribution of the transmitted electrons about the most probable value of their velocity which is given by the Thomson-Whiddington fourth power law; the broadening of the transmitted line had already been established, but this is the first successful attempt to discover quantitatively the law of broadening. The technical name for the dispersion of energy caused by passing through the foil is straggling, and this paper studies the shape of the straggling curve.

The experimental arrangement is to place the radioactive source under the mica foil, in which two or three holes are punched to enable the unstraggled direct beam to be recorded on the photographic plate at the same time as the straggled one for the sake of comparison of the two under identical conditions. A typical microphotometer record is shown in Figure 5, where three of the beta-ray lines emitted by the radioactive source are seen, first, when untransmitted, as very narrow lines, and second, when transmitted by the mica, displaced to the lower velocity side (the right) and much broadened. The correction for the continuous background can obviously be easily made. From this and the known dependence of photographic blackening on number and velocity of the electrons, the variation in number of electrons transmitted for each line is determined and plotted against the difference of velocity between that of each point and that of the untransmitted beam. To give the final shape of the straggling curve, this result must then be corrected for the fact that the initial beam is not ideally narrow, as well as for the overlapping of the two curves. The straggling curve has then finally the shape shown in Figure 6, where whatever the thickness of the film or the velocity of the electrons, the ratios $OF : OP : OB$ are constant and independent of the particular curve under consideration. Thus, if the shape of the curve is determined empirically for one set of conditions, i.e. for one velocity and one thickness, its shape may be computed directly from this curve for another set of conditions. The absolute value of the most probable loss of velocity given in this way is in agreement with the Thomson-Whiddington fourth power law. The work has not been extended to films of other material, so that further work would be desirable to determine how the empirical formula is to be modified for different substances. The result is of value since it gives experimental data by which to test the various atomic theories according to what they predict for the shape of the straggling curve.

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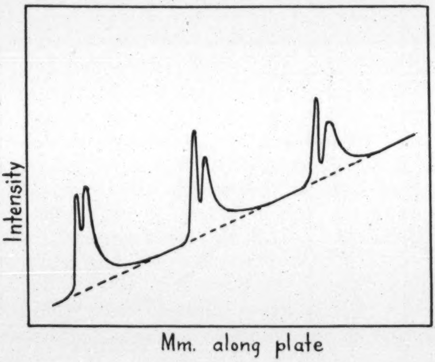
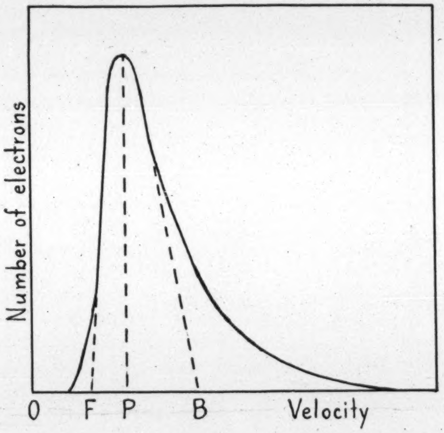
[22] White, P., and Millington, G.: *Proc. Roy. Soc.*, **120**, 701, 1928.

FIG. 5.



Conclusion. From the work of these four investigators we see that many of the characteristics of electron transmission through thin films have been established over a considerable velocity range. Their decrease in velocity has been found qualitatively and quantitatively; even with high velocities the transmitted beam has been shown to be somewhat wider than the initial beam and with lower velocities this widening becomes more and more noticeable. In two of the papers there has been an attempt to discover some effect on the distribution of the emergent electrons in the neighborhood of an x-ray level and no effect has been detected. In one case, although a focussing method was used, the electrons were detected photographically and the weak line which might be expected would not easily be found since long exposures fogged the plate. Schonland, on the other hand, does not use a focussed beam of electrons, and his method would not seem to be relatively sensitive enough to hope to detect this necessarily very small effect.

FIG. 6.



As regards the dependence on angle, there is surprisingly little authentic work on the measurement of the variation of the number transmitted with angle at constant velocity, or with velocity at a given angle. Chadwick and Mercier,[23] for instance, studied the relative number of very fast electrons scattered through a given angle, but the apparatus did not admit of changing this angle, and since the source was radioactive the velocity was necessarily constant. They did vary the thickness of the film and found within what thicknesses the scattering could be considered single. Not only did they vary the thickness but they used different elements for the film—aluminium, copper, silver and gold. In this way the dependence on the thickness and material of the number of electrons scattered nuclearly through a given angle has been found to agree with theoretical considerations. In all cases the angles of scattering were not close to zero degrees.

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[23] Chadwick and Mercier: *Phil. Mag.*, **50**, 208, 1925.

Further work on the variation of the number scattered with angle and velocity has been carried out by Neher [24] who used very high electron velocities, up to 145 kilovolts, and studied the angular distribution between 90° and 180° of those electrons which had suffered no change in velocity. Both the dependence on velocity and angle agreed with the prediction of Rutherford's classical theory for nuclear scattering—that is, the number scattered was proportional to $1/(\sin^4 \theta/2)$ and to $1/v^4$, at the large angles studied. Mott's quantum mechanical modification gave even better agreement. The dependence on atomic number gave a more rapid rise than could be accounted for by proportionality with the square of the atomic number. The absolute values were of the right order of magnitude.

The paper by Eddy [20] is somewhat disconcerting in that it reports a most probable angle of scattering for each thickness of film such that fewer electrons are actually scattered through smaller and larger angles. However, his films are relatively thick, his electron velocities are high, being of radioactive origin, and the emergent beam is not analyzed for velocity distribution. These results do not agree with Klemperer's remark that it is well known that in the neighborhood of 0° the transmitted electrons have a distribution that may be represented by a Gaussian error function of the angle. This difference is probably bound up with the fact that the electrons are so fast that nuclear scattering predominates and with the fact that the beam is not analyzed for velocity.

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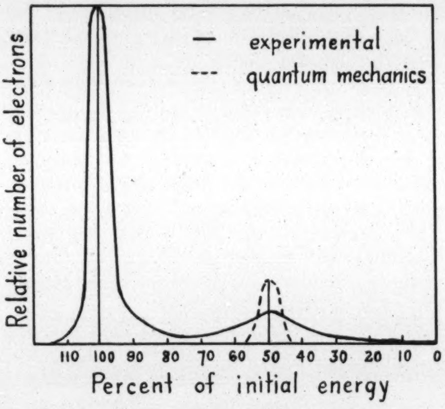
[24] Neher, H. V.: *Phys. Rev.*, 38, 1321, 1931.

Klemperer,[25] in more recent work, using electrons of moderate velocities from a hot filament, has studied the variation in the number scattered with angle and velocity; in this electronic scattering played a part. The electrons were shot in a fine beam from an elaborate hot filament electron gun, passed through the film, and were received at various angles between 30° and 150° by a Geiger counter, which is an ideal instrument for the study of scattered electrons since for small angular apertures in the detector the scattered intensity is so minute that the individual electrons must be counted. It may be noted that Eddy also used a Geiger counter to measure the electron current. The electrons scattered at one angle consist of two groups characterized by different velocities. The higher velocity corresponds to that of the electrons scattered by the nucleus without change of velocity, and the lower velocity to that of those scattered by the electrons in which the velocity is $v \cos \theta$, assuming that only single scattering has taken place. The ratio of the two was of the right order, but somewhat smaller

FIG. 7.



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[25] Klemperer, O.: *Ann. d. Physik*, 3, 849, 1929.

than was to be expected. The dependence on angle was not specifically plotted. More recently [26] the same author has checked up on the ratio of these two velocities by a magnetic analysis of the emergent beam for the angle 45°. The result is shown in Figure 7. Not only was the ratio of the two velocities, or the two energies, that predicted by theory, but the relative number also agreed approximately with that expected on the basis of quantum mechanics. At this angle, his conditions were those of single scattering, both nuclear and electronic.

Thus further information is needed concerning the relation between the number of electrons transmitted by a thin film, the angle of scattering, and the initial and emergent velocities, particularly for small angles of scattering.

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[26] Klemperer, O.: *Phys. ZS.*, **32**, 864, 1931.

Anesthetic Ether. A rather interesting article has been written in connection with the investigations of F. W. NITARDY AND DR. E. C. BILLHEIMER of the E. R. Squibb and Sons laboratories upon the preservation of anesthetic ether in its original purity.

It was not so long ago, considerable stir was caused by finding that much of the ether used in general anesthesia contained small amounts of impurities which had extraordinarily harmful effects upon the patient. Recent investigations would seem to indicate that a very small percentage of peroxide compound in the ether (probably not more than 0.01 to 0.02 per cent.) renders the anesthetized person very susceptible to pneumonia infection. Although the ether may be entirely free from peroxides at the time of packaging, and no doubt is in the majority of instances, peroxide formation seems to take place quite readily under a wide variety of conditions and in various types of containers. Nitardy and Billheimer have found copper-lined tin cans to be the safest form of package for ether and are satisfied that even though kept at elevated temperatures over long periods of time, it will remain free from peroxides or other decomposition products for an indefinite period. Metallic copper possesses the ability to remove from the ether those agents causing oxidation, i.e. peroxide formation.

T. K. C.

EXPERIMENTAL WORK ON THE TRANSMISSION OF ELECTRONS BY THIN GOLD FILMS.

BY

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BARTOL RESEARCH FOUNDATION
Communication No. 67.

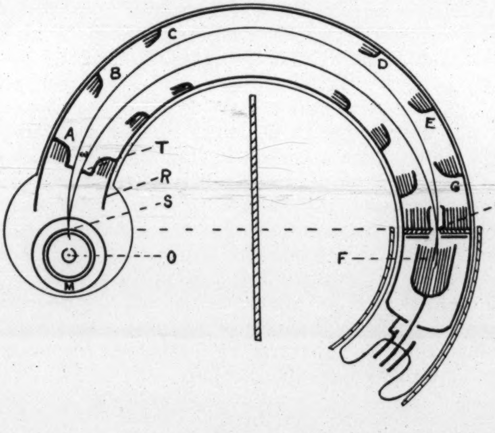
In spite of the enormous amount of experimental work that has been done on the transmission of electrons by thin films, the data on such transmission are not wholly satisfactory nor complete. This is owing in large measure to the extreme complication of the problem and to the seeming impossibility of separating the various factors involved. All attempts to discover some effect on the distribution of velocities of the emergent electron beam in the neighborhood of an x-ray level, e.g. such that a small fraction of the electrons shall have lost a velocity corresponding to the energy necessary to excite a characteristic x-ray, have failed. The present research was undertaken in an effort to find such an effect using the modern high vacuum technique and the hot cathode tube. It also has failed, but further information has been obtained concerning the change in velocity of electrons caused by passing through thin gold films and the dependence, for small angles, of the number transmitted on the angle between the initial and final directions of the beam.

The method used for analyzing the velocity distribution of a beam of electrons was that of applying a magnetic field perpendicular to the direction of motion of the electrons. For that case there exists the relation

$$v = \frac{e}{m} H\rho, \qquad (1)$$

where v is the velocity of the electron in cm./sec., H the strength of the magnetic field in gauss, e/m the ratio of the charge to the mass of the electron, and ρ the radius in centimeters of the arc into which the electron path is bent by the magnetic field. To measure the velocity distribution in an electron beam then, one may either keep H constant and vary ρ, as in the case where the electrons register lines upon a photographic plate, or keep ρ constant and vary H. In the latter case, since it is well known that a beam passing through an initial slit is sharply focussed 180° along the circular path from the initial slit, a second fixed slit may be placed 180° from the first and H varied until the beam has the desired curvature. The resulting small current through the second slit is then measured by a galvanometer or electrometer. This was the method adopted in this research, for by using a large radius of curvature (10 cm.) and a sensitive electrometer for measuring the current, large resolution can be obtained and small variations in the number of electrons transmitted, if they exist, can be detected.

FIG. 1.

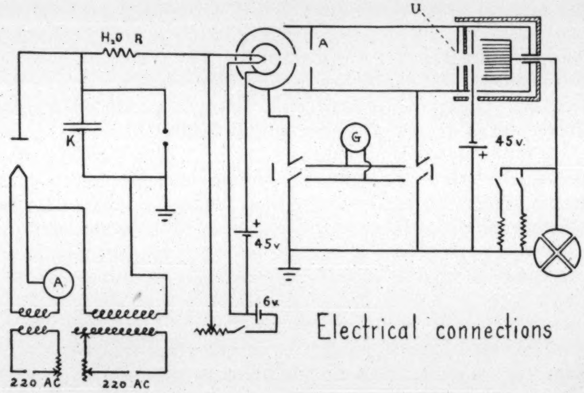


Apparatus. The apparatus used is diagrammed in Fig. 1. The electrons are obtained from a heated tungsten ribbon filament 1 mm. wide and 0.03 mm. thick at O, the heating current being furnished by a storage battery to insure constancy of current and electron emission. This filament is placed along the axis of a copper cylinder approximately a centimeter in diameter (with the return lead as close as possible to the ribbon filament), the cylinder being 45 volts positive to the filament. The electrons pass through a slit *S* and an opening *T* which limits the solid angle, ω, of the beam. A magnetic field *H*, perpendicular to the plane of the paper, bends the electron paths into half circles which come together, thus focussing the beam, at a point *U* diametrically opposite to the slit *S*. The electrons pass through this slit into a Faraday cylinder *F*, which is connected to a quadrant electrometer shunted with suitable high resistances for measuring small currents. The tube itself is of glass, enclosing a semi-circular tube of copper, as shown in the heavier black lines. The vanes *A*, *B*, *C*, *D*, etc., serve to cut out any stray electrons which may be scattered from the walls and are filled with copper foil to make them more successful in trapping the electrons. The Faraday cylinder, too, is filled with copper foil for the same reason.

The velocity of the electron beam is produced by the difference of potential between the heated filament *O* and the slit *S*, the heated filament being at the high negative potential. This difference of potential is furnished by a transformer with kenotron rectifier, where the current in the primary of the transformer is supplied by a local generator to secure as constant a voltage as possible. A glass in oil condenser *K* of 0.04 microfarads capacity permits, with the small currents drawn, of keeping the potential applied to the tube constant to within about 100 volts. The experimental work verified this, inasmuch as in the runs without a thin film, the electrons showed a maximum number for a very narrow range of velocities with very few reaching the Faraday cylinder outside of this main peak. The slit *S* and the entire semi-circular copper tube are grounded, since the filament is at the high negative potential. The potential difference applied varied from 2,000 volts to 20,000 volts. Figure 2 indicates the electrical connections, exclusive of the currents producing the magnetic fields.

The magnetic field is produced by an external solenoid 75 cms. long and 15.5 cms. in radius, which gave an approximately uniform field at its longitudinal center. It was, however, desirable that the path of the electrons should be accurately known and that the electrons, for this reason, should suffer no deviation by the magnetic field until they had attained to their full velocity. Thus the space between the filament and the slit *S* should be absolutely free from magnetic field. If it is not, then, when the magnetic field is varied, for the purpose of bringing on to *U* electrons having different velocities after emergence from the film, the results will be affected in a manner difficult to interpret. For the incident beam of electrons coming from the filament will fail to strike the slit *S* in a manner independent of the magnetic field. For this reason, an additional small solenoid was made of a few turns of heavy copper ribbon and inserted in the tube surrounding the filament. The metal ribbon was perpendicular to the magnetic field, so that the electrons could pass between adjacent coils. A larger copper cylinder attached to the same ground joint as the filament supported this solenoid, with the cylinder and solenoid insulated from each other by mica. The initial slit *S* was carried by this cylinder, being located at the inside edge of the secondary solenoid, and the film holder could be slid vertically in a groove to cover the slit or not, as desired. The electrons were given an initial velocity to pull them away from the

FIG. 2.



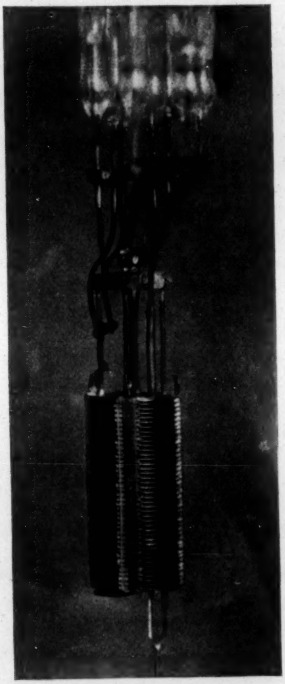
filament by the difference in potential of 45 volts between the filament and the small cylinder inside the secondary solenoid, while the entire large potential difference was applied between the filament and the slit *S*, so that the electrons had their full velocity when passing through the slit. A suitable current passed through this solenoid served to annul, in its interior, the magnetic field produced by the large external solenoid. The compensation was preserved for all values of the current in the larger solenoid by keeping the ratio of the currents in the two solenoids constant. A photograph of the small solenoid and slit is shown in Fig. 3.

The filament, surrounding small cylinder, slit-holding cylinder, and smaller solenoid were all mounted on a single ground-glass joint which turned about the slit *S* as an axis. This enabled one to investigate the velocity distribution of the electrons for different angles between the incident and emergent beams. Inasmuch as the initial slit remained fixed in position the electrons were always focussed on *U*.

The Faraday cylinder which received the electrons was protected as completely as possible. To protect it from electrical leakage currents, its leads, both inside and out, were surrounded by grounded guard rings. To protect it from very low velocity electrons arising from repeated scattering, a second slit was inserted behind the slit *U* and kept at a potential 45 volts negative to the Faraday cylinder itself. This precaution increased the sharpness of the main peak very noticeably and cut down appreciably the number of electrons which in the absence of the film reached the Faraday cylinder for other values of the magnetic field than that allowing the main peak to fall into the Faraday cylinder. To protect it from x-rays, it was carefully surrounded by lead. This included a lead slit behind the slit *U*, since the electrons of the main beam, for values of the magnetic field which were very close to that required to bring them on the slit *U*, would hit the sides of the slit *U* and produce x-rays. When these x-rays hit the Faraday cylinder, their photoelectric effect caused electrons to be ejected, so that in some cases more were ejected than fell into the cylinder and the current was reversed slightly on both sides of the main beam. With the lead slit, this phenomenon was no longer observed.

The current was measured with a quadrant electrometer of sensitivity 800 divisions per volt, shunted with interchangeable high ink resistances, so that the range of measurable current was very wide, from 10^{-13} to 2×10^{-7} amperes.

FIG. 3.



Procedure. The general plan of the experiment was then as follows. The ground glass joint was first turned so that the line joining the filament to the slit S was inclined at some definite angle θ to the axis of the beam which emerged through the slit S and succeeded in passing within the angle ω, an angle of about 3.5°. With the difference of potential applied between the filament and slit, when the film was absent, the current to the Faraday cylinder was measured for various fields in the large solenoid, with, of course, compensating fields in the small solenoid. These measurements gave data on the number of electrons emerging through the slit within the angle ω with various velocities, as calculable from the magnetic field in the large solenoid. In the ideal case, with the film absent, the curve for the Faraday cylinder current plotted against the magnetic field would be one very tall sharp peak with zero ordinates everywhere else for 0°, and zero everywhere for all other angles. In practice curves of the kind indicated by the dotted curves in Fig. 4 were obtained. For 0°, it will be noted that small humps appear on either side of the main peak. These are indeed very small. A simple calculation shows that they may be attributed to a few electrons hitting the vane C which are reflected without loss of velocity into the Faraday cylinder.

Having found the variation of the Faraday cylinder current with the magnetic field in the absence of a film, a similar curve was taken under identical circumstances with a gold film present. The film was brought into place by means of a device operated magnetically by the aid of a small external solenoid, at a considerable distance from the main tube, so that all the adjustments of the apparatus remained unchanged. The full lines of Fig. 4 give the results with a gold film at various angles. The heights of the main peak are seen to be much lower for the transmitted beams. If the scale of plotting for the readings taken without a gold film were adjusted so that the heights of the main peaks were the same as in the case of transmission by the gold film, the side humps of the curves taken without a film would be negligible. Figure 4 shows the results obtained with very thin gold films, of the order of 6×10^{-6} cms. thick. Figure 5 shows a typical result obtained with gold films 1.5×10^{-5} cms. thick, that is, about two and a half times as thick.

The gold films were prepared as indicated by G. P. Thomson.[1] They were sputtered electrically on a smooth rock salt surface. Then the rock salt was dissolved away leaving the gold film floating on the water surface, and the

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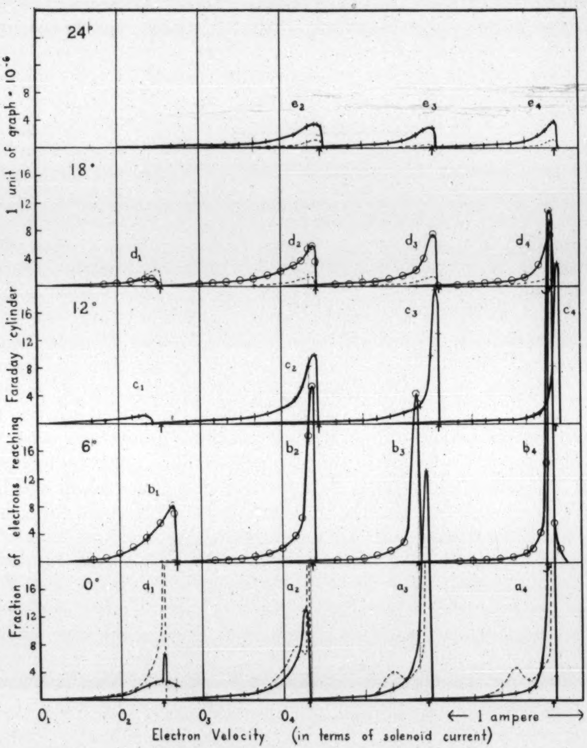
[1] Thomson, G. P.: *Proc. Roy. Soc.*, **125**, 352, 1929.

FIG. 4.



Full curves give results with gold films.
Dotted curves give results without films.
Total initial current for *a*, *b*, *c* and *d* 6×10^{-6} ampere, for *e* 2.5×10^{-6} ampere.
Height of main peak in a_1 2.2×10^{-3}, in a_2 1.3×10^{-3}, in a_3 1.0×10^{-3}, and in a_4 5.3×10^{-4}.
Thickness of gold film in *a*, *b*, *c* and *d* is 6×10^{-6} cm., in *e* 7.5×10^{-6} cm.

One ampere in the solenoid gives a field of 21.5 gauss, which corresponds to an electron velocity of 0.124 *c* cm. per sec. (where *c* is the velocity of light), or approximately to 4.0 kilovolts.

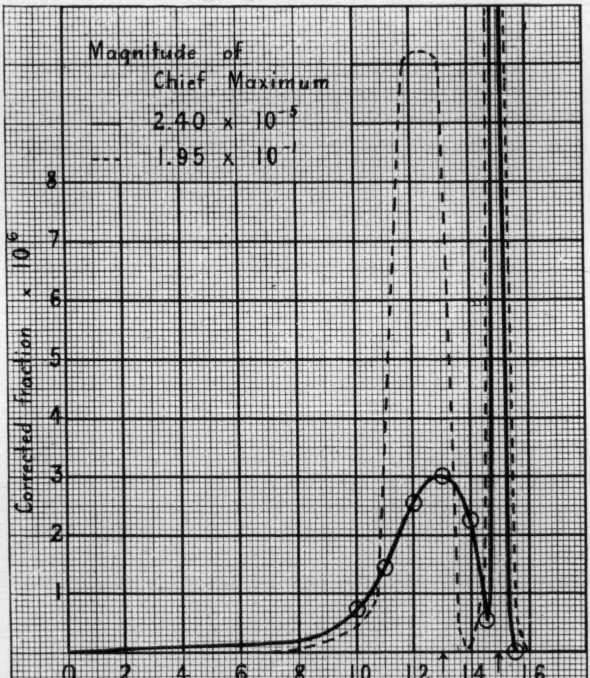
The zeros of the successive curves are shifted progressively to the right to separate the curves better; thus, O_1 is the zero line for the curves with subscript 1, O_2 for those with subscript 2, O_3 for subscript 3, and O_4 for subscript 4.

The zero ordinates for the successive curves vertically are indicated, but in order to conserve space the lower curves are allowed to run into the spaces of the upper curves.

The arrows in each case represent the best determination of the initial velocity.

film was taken up on a metal support with a hole 11 × 3 mm. in it. Even with as small a hole as this, it was not always possible to get the film away from the water surface without

FIG. 5.



breaking it. The thickness of the film was measured by determining its optical opacity with a densitometer, and calculating its density from a formula developed according to the electromagnetic theory, as has been done by one of

G. P. Thomson's students,[2] the published work of whom does indeed involve a very obvious (apparently typographical) error, in that the denominator of the first factor is not squared, as it must be.

Results. The experimental results are collected in Figs. 4 and 5. In each case the dotted curve represents the experiments performed with the film absent. These velocity distribution curves are plotted with the large solenoid currents giving the applied magnetic field, which are proportional to the velocities, as abscissæ and the fractional parts of the transmitted electrons having the velocities given by the abscissæ as the ordinates. The ordinates are thus computed by finding the ratio of that part of the electron beam which, passing through the limiting angle ω, was collected by the Faraday cylinder, to the total electron beam through the initial slit S (the latter quantity being measured by allowing the whole current through this slit to be collected by the copper shield R which was joined to the earthed part of the apparatus through a galvanometer). The ratio is furthermore divided by the relevant value of the magnetic field for each point.[3, 4] This latter correction does not alter the general shape of the velocity distribution cruve, but should be made, as Sharman points out, since for slits of assigned dimensions there is a fairly sharply defined range of values of ρ and hence of v which enters into the current measured for a fixed value of H. The magnitude of this range is obviously proportional to the field intensity. Hence to obtain the true velocity curve (i.e. the relative number of electrons with velocities between v and $v + dv$ plotted against v, when dv is constant over the whole range of v) the ordinates must be divided by the corresponding value of v or of H. The resulting curve will then give not absolute values of the fraction transmitted through that angle, but only relative values.

The Thomson-Whiddington fourth power law[5] for the change of velocity of electrons in passing through thin films of matter has been known for a long time:

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[2] White, Paul: *Phil. Mag.*, **9**, 641, 1930.

[3] Sharman, C. F.: *Proc. Camb. Phil. Soc.*, **23**, 523, 1927.

[4] Dymond, E. G., and Watson, E. E.: *Proc. Roy. Soc.*, **122**, 571, 1929.

[5] Whiddington, R.: *Proc. Roy. Soc.*, **86**, 360, 1912.

$$v_0^4 - v_d^4 = ad \qquad (2)$$

where v_0 is the initial velocity, v_d the most probable velocity of the electron after passing through a film of thickness d, and a a constant which varies with the material of the film and which for gold is given as 2.54×10^{43}.

The thicker film for which results are reported, Fig. 5, shows a displaced maximum in the velocity distribution curve in agreement with expectation. The position of the maximum of this portion of the curve agrees well with the position indicated by the arrow which is calculated as the velocity corresponding to the Thomson-Whiddington formula. The transmitted electrons no longer have a uniform velocity but have a wide range of velocities, as is to be expected. The electrons which did come through without change in velocity may have actually happened to escape any serious collision in their passage through. However, the existence of current corresponding to no loss of velocity naturally raises the question as to whether such current may not have arisen as a result of holes in the film. An examination of Fig. 5 reveals that the total amount of current which has come through with sensibly unaltered velocity is of the order of 10^{-4} of the incident current. Thus to assume that this current is the result of holes is to assume that at least 10^{-4} of the surface of the film was covered with holes. Since the area used was about 0.05 sq. cm., this would involve holes of 5×10^{-6} sq. cm. area, or 2.5×10^{-3} cm. diameter. Such a hole would be on the border line of visibility without a microscope and if made up of two or more smaller holes might have escaped detection. Thin spots would have had a similar effect. On the whole the thinner films used in the later experiments appeared more uniform and were more carefully examined in the densitometer when determining their light transmission as a measure of their thickness. Thus we are willing to admit the possibility of holes in these thicker films, but not in the thinner films.

The typical curve, Fig. 5, shows well the three known effects of the transmission of electrons by a thin film: the decrease in the number of electrons arising from absorption, the shift in the position of the most probable velocity toward lower velocities, and the widening of the velocity peak. All are shown markedly here, since the thin film is relatively quite thick.

Furthermore, the shape of this curve for the displaced hump has interesting significance, inasmuch as White and Millington[6] have derived from their data on the transmission of very fast electrons by thin mica films an empirical formula for the shape of the velocity straggling curve. The White-Millington formula is

$$y = g(x) \quad \text{where} \quad x = \frac{\beta^3}{\sigma}\,\delta(H\rho). \tag{3}$$

The symbols have the following meanings: y indicates the relative intensity of the electron current transmitted plotted against the quantity x as defined, $g(x)$ is the known empirical function the values of which are tabulated for given values of x, β equals v/c for the electrons, σ is the mass per unit area in centigrams per square centimeter, and $\delta(H\rho)$ is measured from the $H\rho$ of the untransmitted beam. This formula holds for the ideal case in which the primary beam is absolutely homogeneous; for actual cases it must be corrected for the breadth of the incident beam. The maximum of the curve determines the most probable velocity of transmission through the film. It becomes of interest to inquire as to the form which the curve relating y to $\delta(H\rho)$ should take for substances other than mica. In the formula as given by White and Millington, variations of σ are to be associated only with variations of the thickness of the mica and not with variations associated with the change of the intrinsic density of the material. The most naive generalization of the formula to the case of other materials would be one in which σ was regarded as the surface density of matter regardless of its material. On such an assumption, the $\delta(H\rho)$ giving the maximum value of x for gold would be given by

$$x_{\max} = \frac{\beta^3}{\sigma_{\mathrm{Au}}}\,\delta(H\rho)_1. \tag{4}$$

On substituting the appropriate values, we find $\delta(H\rho)_1 = 0.63$ amp., as measured in terms of the solenoid current. This value is too large, since the experimental value of Fig. 5 is 0.19 amp. The conclusion is that the effect of change of density from mica to gold, in shifting the maximum—and presumably its effect on the average displacement of $H\rho$ from the initial value—is less than an effect of mere proportionality. The result is not surprising. Insofar as the scattering effect, particularly in the lighter elements, is determined by the electrons, we should expect that doubling the number of such scattering centers by using a film of twice the thickness would, whatever the law of scattering, be more effective than doubling the number without altering the thickness by doubling the number of electrons in the individual atoms, i.e. by using atoms of twice the atomic number. For, in the latter case, many of the electrons, bound more effectively as they are to a heavier nucleus, form less efficient centers for the abstraction of energy from the transmitted beam. Thus although gold has 79 atomic electrons, as compared with, let us say, aluminium which has 13 electrons, it is not 79/13 times as effective in producing energy loss. As a matter of fact, σ for one substance is not quite proportional to the thickness inasmuch as White and Millington give a table of effective thicknesses σ' in terms of actual thicknesses σ where σ' differs from σ by from 2 to 20 per cent of σ.

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[6] White, P., and Millington, G.: *Proc. Roy. Soc.*, **120**, 701, 1928.

If we are to compare the results of Fig. 5 for gold with those of mica, it must be on the basis of some other experiments with gold and mica which involve the same kind of considerations as are involved in the present instance. The most naive theory of the Thomson-Whiddington law would be one in which the coefficient a in Equation (2) was proportional to the atomic number,[7] and so approximately proportional to the density of the material, since the atomic

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[7] Thus, for example, in Bohr's formula the kinetic energy T of the electron in passing through the film is reduced, on the average by the amount

$$\overline{\Delta T} = \frac{4\pi e^4 Z d}{m v^2} \sum_{\nu=1}^{Z} \log \frac{p_\nu}{p_\tau},$$

where e, m, v have the same significance as above, Z is the atomic number, d is the thickness, and the quantities p_ν and p_τ are quantities characteristic of the substance. If this dependence upon the substance is ignored, we have v^4 as a linear function of Z, since $v^2\overline{\Delta T}$ is a linear function of v^4. (Bohr, N.: *Phil. Mag.*, **30**, 581, 1915.)

volumes of most solids are of the same order of magnitude. Such an assumption would lead to the ratio 6.7 for the Whiddington constants of gold and mica, whereas the experimentally found ratio is 3.74. If we should suppose that the effect of doubling the atomic number in the Thomson-Whiddington law is of the same fundamental character as that of doubling the atomic number in the law of straggling, we should conclude that σ is not a constant, representative of the surface density independently of the material, but is a constant whose ratio for different substances is equal to the corresponding inverse ratio of the Whiddington constants. On such an assumption, and using 6.8×10^{42} for the Whiddington constant of mica in conjunction with 2.54×10^{43} as the Whiddington constant for gold, White and Millington's empirical formula for mica would lead to a calculated value of $\delta(H\rho)$ of 0.17 amp. corresponding to the value for the maximum electron current in the writer's experiments for gold. This value is shown by the dotted curves in Fig. 6, the inner curve of which is uncorrected for the width of the initial line while the outer curve is so corrected. Since the full curve represents the data of Fig. 5, the agreement with the position of the experimental maximum is good.

In addition, Fig. 6, computed on the basis of this modified law and with the maximum of the two curves made arbitrarily of the same height for the sake of comparison, shows reasonable agreement between the experimental and theoretical *shapes* of the straggling curve, except that the experimental curve is much wider. This may in part be accounted for by the fact that the angular aperture ω is quite narrow, whereas White and Millington did not limit the angular spreading of their beam so that their results are due to electrons scattered through all sorts of angles. The correction for the width of the initial beam is indicated in the outer dotted curve in the figure and is not at all of the right order of magnitude to account for the discrepancy. The greater spreading of the experimental curve, as compared with the theoretical, is in accord with work done by Millington on the straggling caused by a gold film and reported by Rutherford [8] in his new book according to which it was found "that for the same thickness the peak shift is smaller than for mica, but the actual spreading is greater." The given empirical formula must consequently be further modified to hold for films of various substances. The foregoing conclusions emphasize the importance of a repetition of the straggling experiment with films of different kinds of material with known thicknesses.

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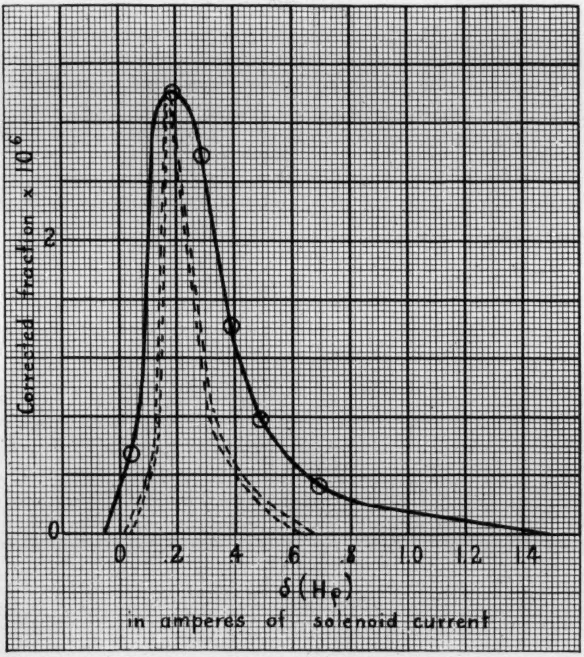
[8] Rutherford, Chadwick and Ellis: "Radiations from Radioactive Substances," 1931, p. 434.

FIG. 6.



In the work with the thinner films of gold, on the other hand, the results are at first sight quite different, as shown in Fig. 4. There is no sign of a large shift in the most probable velocity of the emergent electrons, for at the lower velocities the hump predicted by the Thomson-Whiddington law, provided the constant *a* remained unchanged in value, would be well to the left of the main peak. The maximum of the main peak appears to be shifted a very little, or in the case of the higher velocities not at all. It is, however, noticeably widened on the low velocity side and has a kind of tail extending to very low velocities, thus indicating that there are more electrons with these small velocities than in the blank runs. Some full, or nearly full, velocity electrons appear even in directions making large angles with the direction of the initial beam. It may be noted that at angles other than 0°, the blank runs give almost negligible readings.

It is desirable to give in greater detail the data concerning the displacement of the maximum of the transmitted electron beam from its position in the absence of the film. From the observations obtained in the runs with no film at 0° it was possible to plot a curve connecting the velocity of the electrons, as indicated by the solenoid current, with the A.C. voltage applied to the primary of the transformer. In Table I, in which the data on the position of the maxima of the number of electrons transmitted at various angles are collected, column 2 gives the initial velocity of the electron beam as computed from the A.C. voltage and would be the most reliable court of appeal as regards that velocity, had the voltage been measured with maximum possible accuracy. However, when the observations were taken, the readings for this voltage were not made as carefully as they might have been, as at that time it was not deemed that they would play a very important role in the experiments. In fact, only one reading of the voltage was taken just before the experiment. However, we have another means of arriving at this initial velocity. In the experiments without the film electrons are obtained at angles other than zero degrees, and these are of course scattered from the slits in the apparatus. If they are scattered without loss of velocity, their velocity as measured gives a determination of the velocity of the original beam. The velocities as determined in this way are shown in column 3. (It may be noted in passing that the

TABLE I

Angle.	Velocity from A.C. Voltage.	Experimental Velocity without Film.	Velocity with Gold Film.	Per Cent Loss of Velocity.
0°		.92 amperes	?	
		1.38	1.36	1.45%
		1.66	1.66	.0
		1.98	1.98	.0
6°	1.02	1.02	.99	2.94
	1.42	1.44	1.42	1.39
	1.58	1.62	1.59	1.85
	1.94	1.96	1.98	−1.00
12°	.91	.91	.85	6.60
	1.46	1.49	1.44	3.36
	1.75	1.75	1.73	1.14
	1.98	2.00	2.03	−1.50
18°	.91	.91	.85	6.60
	1.45	1.46	1.43	2.06
	1.74	1.76	1.72	2.32
	1.96	1.98	1.97	.51
24°	1.48	1.50	1.47	2.00
	1.68	1.72	1.72	.0
	1.96	2.01	2.02	− .49

"kick" of the electrometer indicating each of these maxima was too slight to be shown in the blank runs of Fig. 4.) The mean percentage deviation of the velocity as obtained in this way from the velocity derived from the reading of the A.C. voltmeter is 1.2 per cent where the values in column 3 are never less than those in column 2. It may be regarded therefore that the change in velocity arising from reflection from the slits is negligible and that column 3, which as regards its individual readings is founded on more accurate observations than column 2, may be taken as the true representative of the initial velocities. The most probable velocity of the transmitted electrons is given in the fourth column. It is easy to argue that, since the average difference between the initial velocities and the transmitted velocities is the equivalent of about 0.03 amp. and since this average difference is not much greater than that between the two estimates of the velocity of the initial beam (0.019 amp.), the data give evidence that the electron velocity is unchanged in its passage through the film. This view point is strengthened by the fact that in six of the nineteen instances quoted, there is either no loss of velocity or an impossible gain in energy. The largest discrepancies are to be found at the lowest velocities; one may attempt to explain this difference on the basis that for low velocities a slight change in voltage makes an especially large change in the corresponding solenoid current, or that the A.C. voltage readings, not being checked by frequent readings, involve large errors.

However, a closer examination of the data presented in Table I shows that for each angle there is a tendency towards a progressive decrease in the loss of velocity, as the velocity of the electrons increases. At the higher velocities the change in velocity is not sufficient to be detected with this apparatus, whereas at the lower velocities it is too great to be accounted for by experimental error. The one exception to this is for the low velocity curve at 0°, where a study of Fig. 4 shows a small peak having exactly the same velocity as the initial electrons. This is, however, superimposed on a broad hump which one may easily imagine to have a maximum at 0.85 amp. in agreement with the results at 6° and 12°. In attempting to explain this displaced hump for this lowest velocity on the basis of the Thomson-Whiddington law and using the accepted value of the constant a in Equation (2) we immediately encounter trouble, for the displaced maximum should appear close to the origin, that is, the electron beam would be expected to have lost almost its entire velocity instead of the 6.6 per cent. found here. This would lead then to the entire rejection of this possibility, were it not that we recall that Terrill[9] working between 25 and 51 kilovolts obtained for gold a value of a of 11.2×10^{43} whereas the accepted value 2.54×10^{43} is that found by Whiddington[5] in the range 8–22 kilovolts. A similar discrepancy is found for aluminium. The lowest velocity (0.91 amp.) reported in this paper corresponds to a voltage of 3.4 kilovolts, and so lies below the range studied by Whiddington. Data on the variation of a, if this variation does exist, are lacking in the literature. However, it seemed possible to get an approximate check on the order of magnitude of the results obtained here by comparing them with the experimental data given by Chalmers.[10] For thin films of aluminium of 0.0001 gm./sq. cm., he quotes the following experimental results:

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[9] Terrill, H. M.: *Phys. Rev.*, **22**, 101, 1923.

TABLE II.

β.	$d\beta$.
.5	.0009
.4	.0023
.3	.0050
.2	.0106

where β is the initial velocity divided by the velocity of light and $d\beta$ the change produced by transmission through this thin film of aluminium. Luckily the surface density of the thin gold films 6×10^{-6} cm. thick used in this work is 0.000116 gm./sq. cm. and so is very nearly equal to the surface density of his films. The data obtained in these experiments may consequently be correlated with the results of Chalmers for aluminium on the basis of the same assumption that we have already found satisfactory for correlating the results of the thicker film of gold with those of White and Millington for mica. Thus in order to make the comparison, Chalmers' data must be divided by the ratio $a_{\text{Al}}/a_{\text{Au}}$. Curve 1 in Fig. 7 has been plotted directly from Chalmers' data, with the velocities interpreted in terms of our solenoid current and the changes in terms of per cent; curve 2 has been divided by the ratio of the a's and so may be compared directly with our experimental points which are indicated in the figure. The agreement as regards order of magnitude is good. The points for which we have reported no loss of energy would from this corrected curve have too small a loss for sure detection with this apparatus. There are then two possibilities open: to assume that there is always some loss of velocity in passing through a thin film, although it may be too small to be surely detected by the available apparatus, or to assume that with thin films and sufficiently high velocities there is actually no loss of velocity. The experiments reported here cannot decide between the two. Chalmers' data would suggest an asymptotic approach to the x-axis. G. P. Thomson's experiments[11] indicated less than 1 per cent change in velocity of the diffracted electrons, and diffraction theory would suggest no change in velocity. Thus there is evidence on both sides. This work does lead, however, emphatically to the point of view that the Thomson-Whiddington fourth power law is not valid, if the constant *a* be kept rigorously unchanged, for all velocities and thicknesses of film; the constant *a* may perhaps be a function of the velocity of the electrons but not of the thickness of the film. The data reported here are not sufficiently accurate to attempt to determine *a* as a function of the initial velocity of the electrons.

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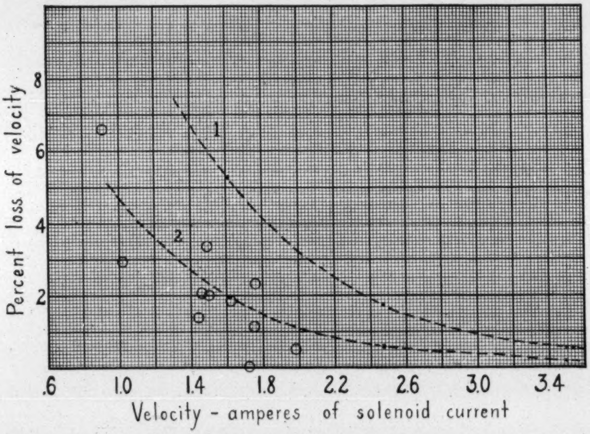
[10] Chalmers, J. A.: *Proc. Camb. Phil. Soc.*, **26**, 252, 1930.

[11] Thomson, G. P.: *Proc. Roy. Soc.*, **119**, 651, 1928.

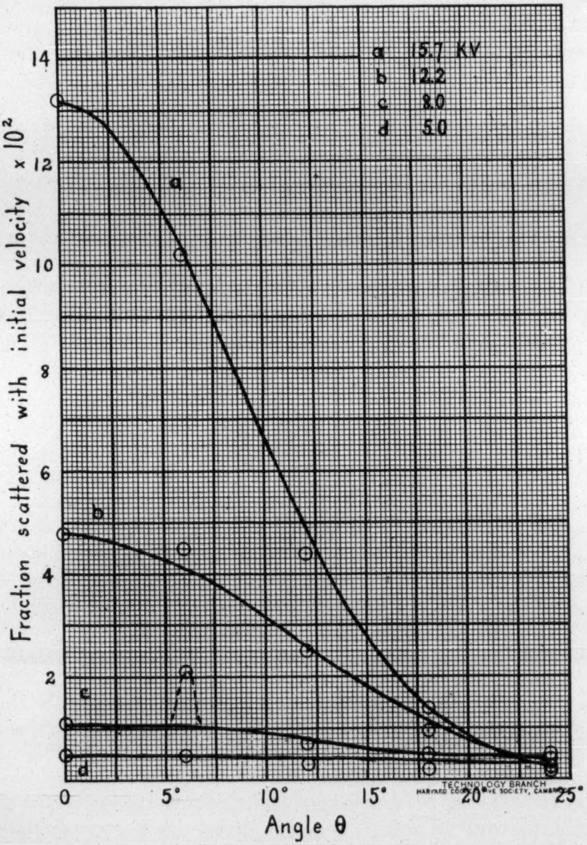
FIG. 7.



Comparison with Chalmers' data

The experimental variation of the fractional part of the electrons scattered with the most probable change in velocity at various angles is given by the points in Fig. 8. The ordinates here represent the relative number of electrons reaching the Faraday cylinder at a given angle θ for a given applied voltage divided by the relative number at 0°, with the film absent, using the same voltage, where we define relative number as the ratio of the Faraday cylinder current to the total current passing through the slit or film to *R*.

FIG. 8.



O–experimental points $y = y\,e^{-\frac{\theta^2}{2\lambda^2}}$
– –Gaussian error curves

Where multiple scattering is concerned—and of course, multiple scattering is involved in these experiments because of the small angles of deflection—it has been well known for some time that the number getting through with a given velocity is a Gaussian error function of the angle of deflection, i.e.

$$\frac{N(\theta)}{N_0} = \text{const. } e^{-\frac{\theta^2}{2\lambda^2}} d\Omega. \qquad (5)$$

The points in Fig. 8 may therefore be studied to see whether they lie on Gaussian curves. To test this, the full curves in Fig. 8 have been drawn as Gaussian error curves, and they are seen satisfactorily to represent the experimental results. The Gaussian constant λ is very nearly equal to the root mean square angle of deviation and may be taken equal to it, provided that $e^{-\frac{\pi^2}{2\lambda^2}}$ is negligible, which is true for $\lambda \leq 0.2$, for example. Moreover, λ will depend upon the velocity of the electrons and upon the density of the film material, the exact form of the relation being dependent upon the law of scattering assumed for the individual collisions. It would be preferable to deduce the form of the analytic dependence of λ on these factors from quantum mechanics, but unfortunately this has not been carried out. The best one can do, if one wishes to check the order of magnitude and functional dependence of λ, is to refer to classical theory which may be expected, as a first approximation, to give reasonable results. On this theory, which Bothe [12] has carried out, λ comes out inversely proportional to the square of the velocity of the electrons. In Fig. 8, the curves are drawn as Gaussian error curves, with the constant λ in curve *a* adjusted to fit the experimental points, and with λ for curves *b*, *c* and *d* derived from that for *a* on the basis that λ is inversely proportional to the square of the velocity of the particles. In every case the heights at 0° are taken arbitrarily to agree with the experimental values. Table III gives the numerical data which have been presented graphically in Fig. 8:

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[12] Bothe: *ZS. f. Physik*, **4**, 300, 1921; **13**, 368, 1923.

TABLE III.

KV.	$\beta = v/c$.	Experimental.		$\lambda\beta^2$.
		Radians.	Degrees.	
15.7	.243	.148	8.5	.00875
12.2	.216	.186	10.6	.00870
8.0	.176	.282	16.2	.00875
5.0	.140	.446	25.6	.00875

The product of β^2 and λ is seen to be constant, thus verifying the variation with velocity. This theory of Bothe's has not been carried out far enough in the case of electronic scattering to give much idea of the order of magnitude which one would expect for λ.

The application of theories of scattering to the experimental data is complicated by the fact that classical theories are apt to lead to an infinite ratio for the number scattered per unit range of angle at zero angle to the total number incident. According to the unpublished conclusions of Swann, this difficulty is of a purely mathematical nature, resulting from a failure of the scattering formulæ to represent the facts as regards very small angles of scattering, this failure having nothing to do primarily with the assumption as to the nature of the scattering entities, but involving rather the fact that the distance between the scattering entities is finite, whereas for zero scattering an infinite distance from the scattering entities must be assumed, which infinite distance is not consistent with the assumption of a finite number of scattering entities per cubic centimeter. The same difficulties occur even in wave-mechanical formulæ, although here the assumption involved appears in another form.

However, in spite of the difficulties of applying the theoretical formulæ to the calculation of what actually happens to electrons scattered in the neighborhood of zero degrees, we may glean some information from them. Experimentally we have found that the electrons have lost some velocity in passing through matter and that the emergent velocities are much less homogeneous than the incident. We have postulated that such a result must occur if multiple electronic scattering takes place. From a study of the theory of double electronic collisions we shall show qualitatively that the velocity distribution curves found experimentally are in line with what would be expected from a theory of multiple collisions.

On the simplest view of scattering, namely scattering by a single entity, we should expect that, if the loss of energy by the transmitted electrons were shared with the single entity, the loss of energy would be a definite function of the angle of scattering. The loss of energy will be appreciable only if the mass of the scattering entity is not much greater than that of the impinging electron and will be detectable in the case of electronic scattering, but not of nuclear scattering. Thus in the case of the collision of two like bodies the mere consideration of the three equations representing the conservation of momentum and the one equation for the conservation of energy, being four equations in all, is sufficient to determine the change in energy of the incident electron, the change in energy of the scattering electron, and the two angles which determine the direction of motion of the scattering entity, the direction of motion of the incident electron having been designated initially. There is, of course, a range of 3.5° in the angle of scattering in the beams corresponding to the various curves whose mean angle of scattering is recorded. However, this range of 3.5° is much too small to be associated with the variation in velocity losses indicated by the curves. It has been suggested by Dr. Swann that in so far as single scattering would lead to a unique loss of velocity which is a function of the angle of scattering only, i.e.

$$\Delta v = v_0 - v_d = v_0 \cos \theta, \tag{6}$$

the existence of a distribution of velocity loss, such as is indicated in Fig. 4, calls for something other than single scattering, and that the general form of such a curve might be duplicated by a consideration of a simple case of double scattering. In carrying this out, we have restricted ourselves to the special case of uniplanar scattering. If we keep the final angle of deflection constant, but vary the angles of deflection arising from the two constituent collisions, the minimum loss of energy occurs when each of the two collisions impresses on the impinging electron a change in direction one-half that of the total change. Using the classical law of electronic scattering, which holds as a first approximation for the more recent formulæ based on quantum mechanics, the velocity distribution arising from double collisions in the case of electrons having suffered 18° deflection is shown by the second curve in Fig. 9. The left hand point corresponds to the number of electrons having lost the minimum velocity which we have noted to exist.[13] By far the greatest number of electrons have lost a velocity close to that which would have been lost in the case of a single collision giving the same final deflection, but the maximum is shifted a little toward the smaller energy loss. This effect is magnified in these curves since to avoid the infinite number scattered at zero degrees given by the theory, it has been assumed that a constant number of electrons are scattered in the range $-2°$ to $+2°$. This would mean that the electrons of the gold atoms were much closer together than 10^{-8} cms. whereas actually their distances must be of this order. Thus to correspond to actual conditions we should take the angular range in which a constant number of electrons is scattered as from $-5'$ to $+5'$. Such a procedure would still give an increase in width of the peak and a displacement towards the smaller energy losses, but both of these changes would be too small to be detected in diagrams of this size. The third curve of the figure gives a rough approximation, on the same grounds of uniplanar scattering, of the velocity distribution of electrons having suffered three collisions before emerging from the film. The widening and displacement are again in a direction away from that of single scattering and towards that of the experimental curve, d_3 of Fig. 4, which is presented for the sake of comparison. The two curves agree qualitatively with respect to their general shape. In fact the agreement is as satisfactory as could be expected on the basis of a theory which is acknowledged to be a much simplified representation of what actually takes place. In practice the experimental curve would only be obtained after the

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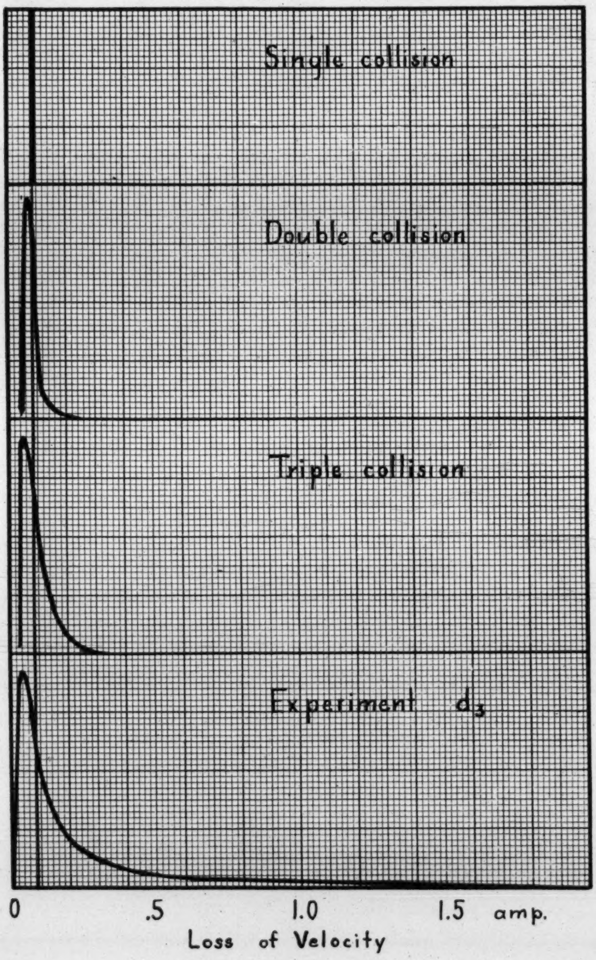
[13] A strict mathematical working out of the two-dimensional case leads to a sharp infinite peak in the neighborhood of the minimum loss of energy. This peak is very close to the main peak which, incidentally, is infinitely high for the classical formula and merges with it. However, since its occurrence is a function of the two-dimensional nature of the problem, it has been omitted in Fig. 9.

FIG. 9.

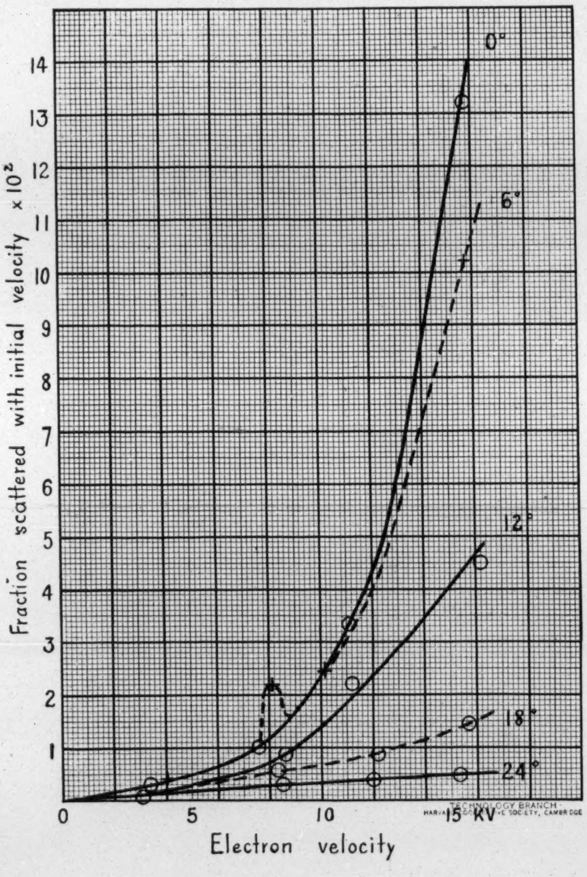

Single collision
Double collision
Triple collision
Experiment d₃
0
.5
1.0
1.5
amp.
Loss of Velocity

consideration of a very considerable number of collisions. The recoil electrons which have been neglected in this theory would raise the number of electrons having lost large amounts of energy, so that the difference between the experimental curve and the theoretical is in the right direction as far as the large energy losses are concerned. Of course, large amounts of loss of energy may occur by a production of x-rays, but the efficiency of this process is far too small to have much bearing upon the explanation of curves. For the loss of energy by the promotion of transitions will be very rare and "should occur to an appreciable extent only with the K electrons of heavy elements, such as gold" (l.c., **8**, p. 236) provided that the velocity of the primary electrons is sufficient to excite the K lines of the element,—which is not true here.

The variation with voltage of the number of electrons scattered at the various angles is given in Fig. 10, where the ordinates again represent the fraction relative to the zero angle direct beam as in Fig. 8. According to the Thomson-Whiddington fourth power law, with the constant given for gold, for films of this thickness, no electrons should get through if they have a velocity less than 3.5 kilovolts. Quantum mechanics, however, gives a finite probability for phenomena which are totally forbidden on the classical theory, so that this is not surprising. Furthermore, the Thomson-Whiddington law is a law of averages and so even in its more rigorous classical derivation does not cut out the possibility of some electrons losing somewhat different velocities. No satisfactory analytical expression for the relation between the fraction transmitted with the most probable velocity and the velocity of the particles has been found. It is to be noted that at small angles the number transmitted increases with the velocity. If we were to attempt to find a suitable analytic expression, there would be involved the reflection coefficient of electrons from gold and the absorption coefficient of electrons in gold, the former of these being perhaps the more important.

One interesting feature of these curves must be noted. In the case of the 6° angle, for about 8.0 kilovolts (computed from the value of the magnetic field required to bend the

FIG. 10.


Fraction scattered with initial velocity × 10^2
14
13
12
11
10
9
8
7
6
5
4
3
2
1
0
5
10
15 KV
Electron velocity
0°
6°
12°
18°
24°
TECHNOLOGY BRANCH

electrons of the main beam into the Faraday cylinder), there is a hump in the curve showing that more electrons come through at 6° than for slightly larger or smaller angles. The same thing appears when plotting the number of electrons in the main peak against the angle made with the direct beam as in Fig. 8. It is of interest, that according to the measurements of G. P. Thomson,[14] for gold, there is a diffraction ring at 6° for this voltage. The de Broglie wavelength h/mv is 1.33×10^{-9} cm. According to the theory developed in Thomson's paper, the angle of deviation φ is

$$\varphi = \frac{n}{a}\frac{h}{mv}\sqrt{h^2 + k^2 + l^2} \tag{7}$$

where n is the order of the diffracted beam, h, k, and l the crystallographic indices, and a the grating constant, which for gold is 4.20×10^{-8} cm. If the diffraction is from the face (113), φ is 0.105 radians or 6°. The agreement is then startlingly good. It is probable that if the region of smaller angles had been studied more thoroughly in smaller angular steps more of these diffraction humps would have been detected. In another set of curves which have not been particularly reported here (since the film was partly broken), electrons of 12.0 kilovolts velocity gave unexpectedly large transmission again at 6°, and this was found similarly to be reasonable if reflection occurred from the (400) plane.

Conclusion. These experiments on the transmission of electrons of 2,000 to 20,000 volts velocity by thin gold films have given the following results:

(*a*) In the case of gold films 1.5×10^{-5} cm. thick, the Thomson-Whiddington law for the average value of the velocity of the transmitted electrons holds, a law based on the average effect of a large number of electronic collisions. The velocity distribution of the transmitted beam is much wider than that of the incident beam. The shape of this straggling curve is similar to the empirical curve developed by White and Millington for mica, but is wider and less displaced than that predicts. It has been shown that this smaller displacement with films composed of heavier atoms but having the same surface density is to be expected on atomic theory.

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[14] Thomson, G. P.: *Proc. Roy. Soc.*, **117**, 600, 1928; **119**, 651, 1928.

(*b*) In the case of gold films 6×10^{-6} cm. thick, the shift in the most probable velocity of the transmitted electrons is very small and at the higher velocities practically non-existent. The widening of the velocity distribution curve is readily observable, although it is not as marked as with the thicker film. The order of magnitude of the loss of velocity is in agreement with that reported by Chalmers for aluminium when a correction is made for the change of element, but is much smaller than would be accounted for by the Thomson-Whiddington law. One would not, of course, expect this law to apply to such thin films, but the sense of the departure from it is of fundamental importance. The evidence presented leads to the conclusion that the constant of the Thomson-Whiddington law is in reality a function of the velocity of the incident electrons, increasing with an increase in their velocity. The data presented were not sufficiently precise to attempt to formulate a law of dependence of the constant on the velocity. It may or may not depend as well on the angle of deflection and the thickness of the film. The loss of velocity and the widening of the velocity distribution of the transmitted electrons can be explained on the basis of multiple electronic scattering; even on the basis of double and triple collisions the general shape of the velocity distribution curve is obtained. The fraction of the initial beam transmitted with the most probable velocity for a given applied voltage may be interpreted as a Gaussian error function of the angle of scattering. The root mean square angle of scattering then seems to be inversely proportional to the second power of the velocity, as given by Bothe's theory. Evidence was also noted for the diffraction of electrons in the abnormally great transmission at one angle for electrons of a given voltage.[15]

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[15] The main conclusions of this paper have already been presented for the writer in the Report of the Director of the Bartol Research Foundation to the Membership of the Franklin Institute in the JOURNAL OF THE FRANKLIN INSTITUTE for December 1930.

EVIDENCE FOR THE CONSTITUTION OF THE ETHER.

BY

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Having, by means of Newton's bucket, given evidence for the *existence* of the ether,* let us cast about for evidence of its *constitution*. We have evidence that it is, but what evidence have we of what it is like? Suppose we begin our quest by defining the ether rather vaguely. We may define it as that medium or frame of reference relative to which a given change of velocity in heavy bodies requires more force to produce than in light ones. No material body conforms to this definition, though for ordinary terrestrial bodies in man's immediate environment, the earth appears to do so; but both the rotational and translational experiments with Newton's bucket show that there must be something which conforms to it. In these experiments it was always the lighter body, the suspended bucket, which gave evidence of change of motion relative to a third frame. The earth and the heavy door were quite successful in resisting change in their motion. Yet the force applied to each was the same, since the impact was mutual, and action equals reaction. Moreover, it was plain that the frame of reference, or inertial frame, or medium or ether, or whatever it may be called, with which matter reacts inertially was something pervading both bodies employed in the experiment because the whole mass of each was involved and not the surface merely. The resistance opposed to change of motion in a body immersed in a viscous liquid, like that which opposes the acceleration of a boat in water, is a surface phenomenon of friction. The water does not react with every particle of the boat, but only with the particles on the surface in contact with the water. This is not so with the resistance opposed to change of motion involved in inertial phenomena. Every particle, whether on the surface or inside the body, takes part in the reaction, showing that whatever is reacting with the body pervades it, somewhat as dissolved sugar pervades water. Matter and ether in fact behave like mutually pervading systems.

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* See this JOURNAL, Vol. 213, p. 421.

Now Kaufmann, early in the present century, performed an epoch-making inertial experiment on the fast moving electrons of which cathode rays are composed. By means of a magnetic field he deviated them from the straight path in which they would otherwise have moved, and discovered a curious fact, namely, that the amount of the deviation was less than that calculated from the laws of Newtonian mechanics. In short, the resistance to change of direction of motion was greater than indicated by theory. This resistance, of course, is simply another name for the inertia of the electron, and he found that the inertia, instead of remaining constant, as previously assumed by all physicists, increased with the velocity of the electron according to a certain law which may be expressed thus: If v is the velocity of the electron and c the velocity of light, and if I_0 is the inertia when the velocity is zero and I_v the inertia when the velocity is v, then the inertia increases with velocity (hence is *relative* to velocity) according to the law:

$$I_v = I_0 \frac{1}{\sqrt{1 - \frac{v^2}{c^2}}}.$$

This result was later confirmed by Bucherer and others, and physicists are now agreed that Kaufmann's law of increase of inertia applies, not alone to electrons, but to all material bodies. Thus Kaufmann's experiment affords information about the nature of inertia not afforded by the experiments with Newton's bucket. It informs us that whatever is resisting the change of motion of bodies resists it much more at high velocities than at low ones, and that the increase in resistance follows a very definite law which involves the velocity of light c. This is a very significant fact and affords a highly important clue, for there seems no *apriori* reason to conclude that the inertial field or ether which pervades bodies and resists their change of motion should have any particular connection with the velocity of light unless it is something which resembles light more than we have been in the habit of assuming. Moreover, it makes clearer than ever that the third frame of reference revealed by Newton's bucket is no imaginary or formal one, which in some mystic manner is due to the mere presence of the heavenly bodies, as Mach suggests and Einstein controverts, since its resistance to change of motion increases with velocity. And this leads us to inquire whether there is anything known to science whose resistance to change of motion would, on independent grounds, be expected to follow the peculiar law discovered by Kaufmann—and the answer is an affirmative one. Larmor discovered that a center of radiation of any kind, irrespective of wave-length, if moved, or convected, relative to its own radiation will resist change of motion in exactly this manner. For, speaking of a system of electrodynamic (radiant) energy freely radiating to space in all directions, he tells us that:

> "The effect of convection on the electrodynamic energy distributed within any regions that correspond in the two states, convected and unconvected, of the system is expressed by
>
> $$E = E_0 \frac{1}{\sqrt{1 - \frac{v^2}{c^2}}},$$
>
> where E_0 is the energy in the unconvected state of the system." [1]

We may call this law of increase of energy Larmor's law. It shows that if we assume material bodies to be composed of centers of radiation radiating freely to space with velocity c, it follows that their inertia will obey the exact law of increase discovered by Kaufmann, when v represents their velocity relative to their own radiation. For the inertia of a body is measured by the energy required to change its velocity by a given amount. If for instance we assume a given particle to have the inertia I_0, then, on the old, classical assumption, to change its velocity from v_1 to a greater velocity v_2 will require an amount of energy equal to

$$\frac{I_0}{2}(v_2^2 - v_1^2),$$

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[1] Larmor, J., "Mathematical and Physical Papers," Vol. 2, p. 447.

because this is the increase of kinetic energy of the particle due to its increase of velocity. But it is plain that if the particle, by virtue of the operation of Larmor's law alone, has more energy at velocity v_2 than at v_1, then more energy will be required to give it the change of velocity from v_1 to v_2 than if this were not the case. In short, the inertia will appear to increase with velocity, and the amount of energy required to change its velocity from v_1 to v_2 will become

$$\frac{I_0}{2}\left[\frac{v_2^2}{\sqrt{1-\frac{v_2^2}{c^2}}}-\frac{v_1^2}{\sqrt{1-\frac{v_1^2}{c^2}}}\right].$$

This of course is neither more nor less than the relativity correction for inertial mass, which, as we see, follows from Larmor's law on the assumption that matter is composed of radiation. It is scarcely necessary to emphasize the light which this fact sheds on the physical interpretation of the relativity theory, since it reveals a physical cause for the correction—something which no other assumption reveals. Yet apparently the relativists have overlooked Larmor's discovery entirely. At any rate, in a rather comprehensive survey of the literature of relativity, I have never encountered a reference to Larmor's law.

There is of course nothing mysterious about the increase of inertia in a convected center of radiation. It is due to the familiar Doppler effect to which all wave motions which radiate outward from centers are subject when the centers are caused to move relative to the waves which they emit. This movement causes the waves in front to shorten up and those behind to lengthen out. But the waves in front shorten faster than those behind lengthen. In fact, those in front approach a length of zero as the velocity of the center approaches that of the waves, whereas those behind only approach a length of twice the stationary value. As the energy density of waves is inversely as the square of the wave-length, it is obvious that the energy of a convected radiating center must, on account of the Doppler effect, increase with its velocity, and Larmor's law simply expresses how much it will increase.

Now what shall we conclude from the agreement of Larmor's calculated result with Kaufmann's observed one? What indeed is usually concluded from the quantitative agreement of calculation and observation? What for instance did Newton conclude when he found that his calculation of the shape of the moon's orbit agreed with observations of its shape? History tells us what he concluded. He concluded that the assumption of the inverse square law of gravitation, on which his calculation was based, was a correct assumption. And does anyone maintain that this conclusion was unwarrantable? Certainly his successors have not so supposed. Would it then be unwarrantable to conclude that the assumption that matter is composed of radiation, from which the agreement of Larmor's calculated and Kaufmann's observed law of inertia is predictable, is a correct assumption? Apparently not. At any rate it would seem reasonable to make this assumption provisionally while we seek for further evidence bearing on the point. Nor have we far to seek. For in the modern theory of wave mechanics and its experimental verification we find independent evidence which is unmistakably confirmatory. This evidence is too technical and extensive for presentation here, but the conclusion to which it leads may be understood by the following quotations from authorities in the field of wave mechanics:

> "The particle is . . . to be regarded as a group of waves and the particle velocity is in the wave theory to be interpreted as the velocity of this group." [2]
>
> "In the way of looking at the question adopted here [in the wave mechanics theory] . . . the material particle is an essential reality and its action is completely determined as that of a singularity in the amplitude of a wave which is propagated." [3]
>
> "If we want to calculate the path of a beam of electrons . . . we must treat the beam as though it were a beam of waves. These waves we call 'deBroglie' waves . . . deBroglie waves have certainly as much physical reality as light waves." [4]
>
> "The evidence that electrons are waves is similar to the evidence that light and X-rays are waves." [5]

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[2] Flint, H. T., "Wave Mechanics," p. 35.

[3] deBroglie, L., and Brillouin, L., "Selected Papers on Wave Mechanics," p. 114.

[4] Mott, N. F., "An Outline of Wave Mechanics," p. 8.

[5] Davisson, C. J., *The Bell System Technical Journal*, Vol. 8, p. 217.

> "From the experimental standpoint an atom is as much a wave as is a quantum of light." [6]
>
> "The rule for finding the wave-length of any particle is to divide the quantum by the momentum of the particle, and this gives the ultimate meaning of the quantum. The rule is true not only for electrons, but also for protons, atoms, molecules, photons and other bodies." [7]

These conclusions, based on an impressive mass of data, both mathematical and experimental, may reasonably lead us to infer that the clue to the cause of inertia given by the Kaufmann experiment is certainly a promising one. But if matter is composed of waves and its inertia has the radiational origin which we suspect, it is clear that the waves, or at least most of them, must be of a super-penetrating kind. Otherwise they could not be propagated throughout the mass of solid bodies and cause every particle thereof to follow Larmor's law. For, as already noted, inertial reactions are not surface phenomena, but take place throughout the mass of bodies. And this reminds us that there is one other kind of reaction equally pervasive, namely, gravitation, a force which Galileo proved was proportional to inertia. Can it be that gravitation is due to a super-penetrating radiation also? If so we may be on the track of the cause of gravitation as well as of inertia. Nor would this be so very surprising, for the two things are very evidently connected in some way. Otherwise they would not be proportional to one another, pervade matter in the same degree, be equally independent of the composition thereof, and equally unsusceptible to screens. The relativists, indeed, go so far as to identify them. Thus Weyl says:

> "Inertial and gravitational masses are identical in nature." [8]

and Einstein agrees with Weyl, for he says:

> "The same quality of a body manifests itself according to circumstances as 'inertia' or as 'weight' (lit. 'heaviness')." [9]

Such identification no doubt is going too far, but it is significant that Newton proved the intensity of gravitation to vary inversely as the square of the distance from the source, just as the intensity of radiation varies, and modern mathematicians have given reasons for believing that it moves through space with a velocity of 186,300 miles per second, which is, of course, the velocity of radiation. Thus Einstein says:

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[6] Johnson, T. J., JOURNAL OF THE FRANKLIN INSTITUTE, Aug., 1930, p. 152.

[7] Darwin, C. G., "The New Conceptions of Matter," p. 99.

[8] Weyl, H., "Space—Time—Matter," p. 307.

[9] Einstein, A., "Relativity," p. 65.

> "The success of the Faraday-Maxwell interpretation of electromagnetic action at a distance resulted in physicists becoming convinced that there are no such things as instantaneous actions at a distance (not involving an intermediary medium) of the type of Newton's law of gravitation. According to the theory of relativity, action at a distance with the velocity of light always takes the place of instantaneous action at a distance or of action at a distance with an infinite velocity of transmission."[10]

Gravitation then is a force which, if not caused by radiation, certainly behaves very much as if it were. Thus from the Kaufmann experiment and the data of wave mechanics we obtain plausible clues as to the nature of inertia, and from the nature of inertia, coupled with the facts of gravitation itself, including the fact that inertia and gravitation are proportional, we are led to a plausible guess about the nature of gravitation. From the Kaufmann experiment as origin, therefore, a chain of coincidences may be followed which gives promise of unravelling some of the deepest mysteries of the universe. But at this point three objections are encountered which require serious consideration.

First, if matter is composed of centers of radiation ceaselessly radiating energy into space, where does all the energy come from? Energy cannot be created out of nothing. Hence if matter had the constitution postulated, it would soon be dissipated into space, and we know that it is not.

Second, if the radiation emanating from these centers is the cause of gravitation, why is not gravitation a repulsive instead of an attractive force. Radiation is known to exert a pressure on bodies upon which it impinges. Hence if the radiation which causes inertia is also the cause of gravitation, bodies should repel, instead of attract, one another. Moreover, matter should produce a slight shadow or screening effect upon gravitation analogous to what an attenuated gas produces upon light, yet no such effect has ever been detected though skilled experimenters have attempted its detection.

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[10] Einstein, A., "Relativity," p. 48.

Third, if matter is composed of centers of radiation expanding outward into space with the speed of light, why do not the Doppler effects caused by the motion of these centers finally bring them to rest? Radiation is known to exert a back-pressure on its source, and as the back-pressure of the shortened waves in front is greater than that of the lengthened waves behind, they should cause a retardation which would prevent the heavenly bodies from moving through space in the unretarded manner that they are known to move.

These objections certainly look formidable, but let us examine them in the light of observed facts and reasonable inferences.

Beginning with the first objection, we shall venture a guess about the constitution of the ether which it is the main object of this lecture to examine. We shall guess that it is composed of a field of super-penetrating radiation pervading space and moving in all directions as the field of starlight and cosmic radiation is known to do. And we shall further guess that it is the absorption of some of this radiation by matter which prevents the dissipation of matter into space. In short, we shall guess that not only matter but ether is composed of radiation, matter being an energy node in the ether and thus a modification of it. Hence the velocity of material bodies will always be a group velocity as maintained by deBroglie and his co-workers. And it does not "cool down" or radiate its substance into space for the same reason that the molecules of air in a room at constant temperature do not cool down. The molecules are constantly radiating radiant heat into space, but they are as constantly absorbing radiant heat from space. In short, they are in dynamic equilibrium with their environment and their radiation is a re-radiation. Similarly all material particles may be assumed to be constantly radiating a super-penetrating radiation into space, but as constantly absorbing such radiation from space. They are in dynamic equilibrium with the ether, or at least approximately so, and their radiation is re-radiation. Their accelerations relative to the ether measure their departure from complete equilibrium. Hence they do not dissipate into space, but maintain their integrity. We may call this guess about the constitution of ether and matter the radiation theory. If it is correct then, our first objection is met, and we know where the waves which the wave mechanics theory calls for, and which cause inertia to increase with motion in the manner discovered by Kaufmann, come from. They come from the limitless reservoir of radiation which pervades space (and matter).

But in meeting this first objection to our explanation of the Kaufmann experiment, we find that unwittingly we have apparently met an objection that besets the wave mechanics theory also. Note for instance what Darwin has to say about the inadequacies of that theory:

> "We might at first sight be tempted to think that we had got a quite satisfactory and complete view of the character of an electron merely as being a wave of very short wave-length. But a little consideration shows that this will not do.
>
> "In the first place we have seen that though a patch of disturbance travels along as an individual with the definite group-velocity, there is always a region round its edges where the disturbance is slowly spreading. There is no way in which a wave can escape this gradual diffusion, and it means that ultimately it will become spread all over space. . . . We may say that the existence of fossils which have preserved their form unchanged for several hundred million years disproves the adequacy of the wave-theory." [11]

The disproof of the adequacy of the wave theory thus set forth consists of two objections thereto: (1) The theory does not explain where the energy comes from which is "spread all over space" in the manner described. (2) It does not explain how material bodies maintain their integrity. The radiation theory meets the first objection by interpreting mechanical radiation as re-radiation from a radiant ether, and it may perhaps be able to meet the second objection by a distinction which it makes between the inertial and the impenetrability "particle," a subject which will be discussed later in connection with the Michelson-Morley experiment. In passing it may be noted that when Darwin speaks of the waves of which matter is composed as spreading "slowly," he is speaking of the group velocity, not the wave velocity. The latter velocity, at least in regions remote from matter, is presumably that of light.

Much stronger implications of the radiation theory are probably involved in the wave mechanics theory than the one here given, but as it would take some time to explain them, they must be passed by.

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[11] Darwin, C. G., "The New Conceptions of Matter," pp. 91, 92.

We may now turn our attention to the second objection—that gravitation should be a repulsive instead of an attractive force—and it becomes evident at once that if the ether has the constitution which has been assumed, this objection loses its validity also. At any rate Newton's inverse square law of attraction follows from that constitution if we make one rather plausible assumption about the manner in which the ethereal radiation is by matter re-radiated.

It is well known to physicists that many substances re-radiate light of a different wave-length from that which is absorbed. This phenomenon is known as fluorescence. It was formerly supposed that fluorescence was rather an exceptional phenomenon, but since improved methods of detecting it have been developed, it has been found that most substances fluoresce to some extent, and the presumption is now considerable that there is no substance in which fluorescence is entirely absent. It would not be unreasonable therefore to assume that in the process of absorbing and re-radiating ethereal radiation by matter a slight change of wave-length occurs. In fact it would be rather unreasonable to assume otherwise, since such a change is the rule rather than the exception. But the absorption of radiation by matter is known to vary with the wave-length, and if we assume fluorescence to result in a form of radiation which is less absorbed by matter than ethereal radiation in general—which is at any rate as likely as the opposite assumption—then it follows that the radiation emanating from matter will be absorbed less by matter than that pervading space in general. If so a deficit of radiation pressure will emanate from every particle of matter in the universe, for radiation only produces pressure when it is absorbed (or reflected). Thus matter would tend to unbalance the uniform pressure of the ether in such a way as to cause material particles to attract one another, since the excess pressure on any body would be always on the side away from neighboring bodies in proportion to their mass, and Lorentz has shown, in his examination of this theory, that this excess pressure follows the Newtonian inverse square law.[12] Lorentz to be sure does not make precisely the assumption here made. He does not assume fluorescence giving a slightly altered form of superpenetrating radiation. He assumes a change of re-radiated wave-length so extreme that actual light and radiant heat would be emitted.[13] Both assumptions result in Newton's law of gravitation, but on Lorentz's assumption terrific heat would be generated and this certainly does not agree with the facts. Lorentz therefore rejected this theory of gravitation, but his particular form of the theory is obviously defective and deserves rejection. Indeed such an extreme assumption as his is not only unnecessary, but out of all accord with the facts of fluorescence as we know them. It would be like assuming that ordinary light on fluorescence would give radio waves hundreds of yards in length instead of fluorescing from blue to red for instance as various substances are known to do. Fluorescence is never known to result in any such change of wave-length as postulated by Lorentz and it is difficult to understand how he came to make such an assumption.

Perhaps the most plausible assumption about the "fluorescent" radiation which causes gravitation is that it is of longitudinal form instead of transverse. Theory requires that such radiation of very great velocity and penetrating power shall accompany all transverse radiation, but none has been found accompanying light though carefully searched for. Moreover, this form of radiation would not only provide the deficit of radiation pressure called for by our theory, but theoretically would be gradually re-transformed into transverse radiation in its passage through space, thus meeting another cosmological requirement. For it is maintained by astronomers that a gravitational force extending to infinity is incompatible with the Newtonian law of gravitation, except on certain very special assumptions, which inherently are rather unlikely. Consideration of the radiational theory of gravitation, however, is too extensive a subject for adequate discussion here. We must therefore dismiss it with this incomplete notice and direct attention to another characteristic of radiation which would be more likely to produce the heating effect assumed by Lorentz than fluorescence or change to longitudinal radiation.

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[12] Lorentz, H. A., "Lectures on Theoretical Physics," Vol. 1, p. 155.

[13] Poincaré, H., "The Foundations of Science," p. 521.

We may surmise that the field of radiation which constitutes the ether is not of uniform wave-length, though no doubt the bulk of its energy is contained in a zone of wave-lengths of a super-penetrating character. It probably consists of a vast spectrum covering far more octaves than the solar spectrum for instance. Indeed we are entitled to suspect that the cosmic rays discovered in recent years may constitute the longer waves of that spectrum—a part of the "infra-red" zone possessing lengths sufficiently great to affect electroscopes. These rays at least conform to the characteristics called for, since they are known to constitute a field of highly penetrating radiation pervading space and propagated in all directions. Now it is not unreasonable to suppose that the ethereal radiation (ether) when impinging on matter may be scattered thereby as other radiation thus impinging is known to be scattered. In short, the ether is presumably subject to a Compton effect, though owing to its super-penetrating character and the minuteness of the particles required to scatter its average wave-length, most of the radiation of which it is composed would doubtless pass through a star or other heavenly body without being scattered. Now it is known that this scattering effect not only increases the kinetic energy of the particle upon which the radiation impinges, but increases the wave-length of the scattered radiation by an amount depending on the angle of scattering. And this renders the radiation so scattered more subject to further scattering than unscattered radiation. The consequence would be that the increase of wave-length thus caused would be cumulative and, in sufficiently large bodies, would, by repeated scatterings, eventually result in radiation of wave-lengths sufficiently long to be scattered by electrons and protons and even atoms, and thus transmit its kinetic energy to them. This would mean that, in bodies sufficiently large, the radiation emitted would become of a wave-length great enough to affect a bolometer. In other words the body would generate a temperature, emit light and radiant heat and become self-luminous, just as Lorentz assumed. But this could occur only in bodies of very large mass where the scattering would be subject to repeated re-scattering. Moreover, the amount of bolometric radiation, though a function of mass, would not be proportional to it, because it would depend, not on the first scattering alone, but on the degree in which re-scattering occurs, and this would itself be greater the greater the mass. Indeed, only an insignificant fraction would reach the bolometric stage in bodies of small magnitude.

While this surmise may be a mistake, since we have no proof that the bulk of the ethereal radiation is sufficiently like light or X-rays to be subject to scattering by matter, yet we are constrained to presume that it may be sound by the behavior of the stars, whose total emission of radiation detectable by astronomical instruments (called the absolute bolometric magnitude) is found to depend on the mass and on little else, and to increase with the mass, not proportionally, but at a greater rate than that of linear proportion. Thus we are told:

> "The total radiation from a star should . . . depend mainly upon its mass, and relatively little upon its size or composition (molecular weight). This conclusion is in excellent accordance with the observed facts." [14]

And again we are informed on the same authority:

> "A giant star lives at a much more rapid rate (so to speak) than a dwarf, its radiation being much greater in proportion to its mass." [15]

This of course is just the way the stars should behave if the source of their energy is the dynamic ether whose radiation, transformed by some process akin to the Compton effect, finally reaches the bolometric stage.

Current theories of the origin of stellar energy assume that the stars are like great unrechargeable storage batteries, deriving their energy from internal sources and gradually running down, the bulk of their radiation coming from the annihilation of the electrons and protons of which their substance is composed. Jeans for example calculates that "The sun's mass is diminishing at the rate of . . . about four million tons a second" [16] from this cause. The radiation theory suggests a different situation. It suggests that the stars are rather huge transformers of energy, turning a portion of the almost limitless supply of radiation which traverses them into wave-lengths to which the eye and the bolometer are sensitive, and hence are not running down at all. It does not to be sure deny that part of the energy comes from other sources. No doubt contraction, meteoric bombardment, the transmutation of elements, and perhaps the annihilation of matter, not to mention other possible factors, may play a part and mask the effect of scattering. But the observed relation between mass and bolometric magnitude is one which certainly suggests the "transformer" theory of the origin of stellar energy much more than the "storage battery" theory.

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[14] "Astronomy," Russell, Dugan and Stewart, Vol. 2, p. 890.

[15] Ibid., p. 917.

[16] Jeans, J. H., "Astronomy and Cosmogony," p. 112.

The latter theory would seem to require that space be thickly peopled with the run-down suns which have passed beyond the red dwarf stage—dark, burned out stars which have accumulated through the immeasurable ages. There is to be sure no proof that this is not the case, since dark stars would be quite generally invisible to human beings. But astronomers in their calculations of the total matter in the universe, or in our galaxy, seem not to assume the existence of any considerable proportion of dark stars, but regard the bulk of dark matter in space as made up, for the most part, and perhaps exclusively, of the fine dust which is supposed to constitute the diffused nebulæ.

The double stars, or binaries as they are called, afford further evidence that the stars are not running down. Many thousands of them are known. Indeed Jeans tells us that:

> "Roughly we may say that of every hundred stars in the sky, about 75 are likely to be single stars, while the remaining 25 will be binary or multiple." [17]

The two "components," as they are called, of a binary system revolve around their common center of gravity; and these components are often of very unequal mass. If stars, like storage batteries, run down, then there ought to be a fair proportion of run down stars discoverable among the binaries, especially among the components of smaller mass, which would exhaust their powers of radiation sooner than the larger components. Yet with one possible exception (β Lyræ) no dark star has ever been discovered in a binary system, though there may be one or two other exceptions among multiple systems, as in the case of ζ Cancri. These exceptions, however, are both scarce and doubtful. The binaries are of three classes—spectroscopic, eclipsing and visual. The first class are non-committal on the issue of the existence of dark stars, since they are very distant, and one component might be dark and yet not be recognized as dark. But among eclipsing and visual binaries a dark companion would be recognizable, and hundreds, if not thousands, have been examined with the care necessary for such recognition; yet Aitkin, a leading authority on binaries, after pointing out that the companions of Sirius and Procyon, which at first were supposed to be dark, turned out not to be, proceeds to say:

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[17] Jeans, J. H., "Astronomy and Cosmogony," p. 24.

> "Dark companions to β Orionis, α Hydræ, and α Virginis have also been suspected from supposed irregularities in the proper motions, but closer examination of the data has not verified the suspicion. Since cases of this kind will probably always be very exceptional, the formulæ for their investigation will not be considered here." [18]

It seems clear that the failure to positively identify a single dark star among the eclipsing and visual binaries (unless β Lyræ should eventually turn out to be such an identification) is a strong argument against the "storage battery" theory of stellar energy, since if the stars are really running down, not one but a fair proportion of burned-out stars should be found among these binaries. If not, why not? Nor is this the only objection against the storage battery theory. That theory indeed is so complicated and doubtful that Eddington remarks:

> "Unfortunately the facts as yet do not fall into satisfactory order, and we are still groping for a clue." [19]

And in seeking to fit the theory to current theories of stellar evolution (which also assume a running-down process) he encounters so many obstacles that he winds up his exceedingly able exposition by saying:

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[18] Aitkin, R. G., "The Binary Stars," p. 101.

[19] Eddington, A. S., "The Internal Constitution of the Stars," p. 297.

> "Somewhere in the present tangle of evolution and sources of energy I have been misled; and my guidance of the reader must terminate with the admission that I have lost my way." [20]

Whether the "transformer" theory of stellar energy suggested by the radiation theory can be made to fit the facts reasonably well is something which would require an extended examination to determine—one far too extended to be undertaken here. Doubtless many difficulties would be encountered. But however serious they might be they could hardly be more so than those which beset present theories. With this inadequate discussion then, we must leave this aspect of the radiation theory, and attempt to meet the other difficulty connected with the second objection, namely, why matter exerts no shadow or screening effect upon gravitation.

The reply of the radiation theory to this criticism is that matter does exert such an effect, but one too feeble to detect by methods hitherto used. If it could be shown that the screening effect of matter on gravitation is absolutely *nil*, it would, as far as I can see, constitute a refutation of the radiation theory of gravitation, since, so far as we know, any physical agency penetrating layer after layer of material which in any way diminishes the power of that agency to produce further physical effects must be subject to a diminution in intensity which increases with the thickness of material penetrated, just as the intensity of light is diminished more by a thick layer of fluorescing gas than a thin one. Complete failure of such a screening effect, therefore, if it could be demonstrated, would disprove not only the radiation theory of gravitation, but any theory which attributes gravitation to a physical agency resembling in the slightest degree any other cause of physical change known to science. It would seem to demonstrate that gravitation is a force acting mystically at a distance, without the intervention of any intermediary means of operation, and this is so unlike the way in which effects are produced by causes in other realms of nature that the presumption is strongly against it. Physicists indeed, relying on experience and the general uniformity of nature, are almost a unit against the theory of action at a distance. That Einstein coincides with this judgment may be ascertained by noting what he says in the citation on page 424 of this JOURNAL, Vol. 213.

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[20] Ibid., p. 392.

Failure to detect a screening effect on gravitation is a refutation of the radiation theory of gravitation of the same kind as failure to detect stellar parallax at the time of Copernicus was a refutation of the Copernican theory of the solar system; and presumably due to the same cause—lack of instrumentalities sufficiently sensitive to detect the effect. If the earth revolves around the sun, its orbital movement should cause a slight shift in the apparent positions of the stars, owing to the change in the angle from which observers on the earth observe those positions at different parts of the orbit. No such shift in the position of any star was observable at the time of Copernicus, and he met the objection by assuming the explanation lay in the great distances of the stars, and that the shift (parallax) would be detectable when instruments of sufficient precision were devised to detect it. Subsequent developments have proved his assumption correct, for stellar parallax is now easily detected for the nearer stars. The absence of detectable gravitational screening is an objection to the radiation theory of somewhat similar character and, so far as I can see, can be met in only one way. We must assume the means of detection thus far employed to be insufficiently sensitive, and trust to future developments to confirm the assumption.* Any other assumption indeed would seem to force us to postulate action at a distance, which is a decidedly improbable hypothesis. We may now turn to the third objection, enumerated on page 80.

To meet the objection afforded by the unretarded motion of the heavenly bodies I direct your attention to the behavior of a gas at constant volume. No matter how high the temperature, or how attenuated the gas, the temperature will remain constant unless by conduction or radiation the gas can transmit heat beyond the volume that it occupies. This means that the molecules of which it is composed do not change their velocity, for the temperature is proportional to the square of the average velocity, and if it remains constant, the average velocity must remain constant also. And of course if the molecules do not change their velocity they must be unretarded. Yet all of them are moving through a space filled with (heat) radiation moving in all directions and steadily absorbing and re-radiating it. Hence they are subject to the same Doppler effects to which matter moving through, and exchanging energy with, a radiant ether would be subject. Some compensating influence therefore must be at work. Retardation by emitted radiation has been shown by Larmor [21] not to occur, because of the compensating influence of loss of mass in the radiating body; but reasoning based on the law of the conservation of momentum indicates a retardation by absorbed radiation. The theory of relativity, on the other hand, would appear to be in conflict with retardation by either class of radiation. Thus Eddington tells us:

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* Majorana claims to have detected a screening effect, but his results have not been confirmed by other physicists.

> "It has often been suggested that the stars will be retarded by the back-pressure of their own radiation. The idea is that since the star is moving forward the emitted radiation is rather heaped up in front of it and thinned out behind. Since radiation exerts pressure the pressure will be stronger on the front surface than on the rear. Therefore there is a force retarding the star tending to bring it gradually to rest. . . .
>
> "But according to the theory of relativity 'coming to rest' has no meaning. A decrease of velocity relative to one frame is an increase relative to another frame. There is no absolute velocity and no absolute rest for the star to come to. The suggestion may therefore be at once dismissed as fallacious." [22]

This passage to be sure refers to emitted radiation only, but the reason given for non-retardation applies equally to absorbed radiation, since 'coming to rest,' or change of velocity, relative to absorbed radiation would involve absolute velocity as much as if the radiation were emitted. Eddington's conclusion is based on the dictum of relativity that the relative velocity of matter and radiation is unalterable by any motion of the former. Hence it applies to absorbed as much as to emitted radiation.

The theory of relativity is no doubt correct in concluding that there is compensation under the conditions postulated, since similar conclusions based on its equations have been found dependable; but the physical cause which operates to compensate absorbed radiation seems not as easy to identify as that which compensates emitted. I have been unable to locate any authority who has identified it. Nevertheless the inability of the molecules of an attenuated gas to convert their kinetic energy into radiation through retardation by Doppler effects proves that such compensating forces must be at work. The mechanism is unknown, but we might perhaps surmise that they are the accelerating forces (gravitation, cohesion, etc.) which material bodies exert upon one another, since these, so far as we know, do not diminish with velocity, whereas the retarding influence exerted by Doppler effects diminishes with velocity in such a manner that it can only bring radiating bodies to absolute rest in infinite time. An average velocity different from zero would therefore be required of bodies in equilibrium with radiation. Whether such accelerating forces are able in all cases to provide the compensating influence which results in the behavior of an attenuated gas has apparently not been determined by mathematicians, but in any event the known facts of that behavior are sufficient to answer the objection that the heavenly bodies would be retarded by the Doppler effects to which, according to the radiation theory, they are subject, since, according to that theory, they are the analogues of the molecules of an attenuated gas, and may be expected to exhibit an analogous behavior. So much then for the third objection. Like the others, it disappears on examination. There are indeed some reasons for believing that in the Doppler effects required by the radiation theory may be found the explanation of a peculiarity of the stellar system for which no other explanation appears to have been suggested. These reasons may be briefly indicated as follows:

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[21] Larmor, J., "Mathematical and Physical Papers," Vol. 2, p. 449.

[22] Eddington, A. S., "The Nature of the Physical World," pp. 58, 59.

The retarding effect of absorbed radiation on the heavenly bodies would, at high velocity of such bodies, be very great, since the energy of the retarding radiation encountered in front would be vastly greater than that of the accelerating radiation received from behind. In fact, at material velocities approaching that of light, the retarding energy would theoretically approach infinity—and this alone would fix the maximum velocity of bodies in the universe as the velocity of light. And it is a significant fact that the fastest moving electrons move with a velocity approaching that of light, but never actually attain it. The theory of relativity also fixes the velocity of light as the maximum material velocity in the universe. At low velocities on the other hand, the retarding tendency would be slight, approaching zero as the material velocity approaches zero. If then we assume the magnitude of the compensating forces in the interstellar spaces to be of the modest intensity exhibited by the force of gravitation (and this would seem a reasonable assumption) it would follow that stellar velocities could never be greater than a small fraction of that of light, since high velocities would require enormous compensating forces to maintain them. Thus the equilibrium between the retarding Doppler forces and the compensating accelerating forces, whatever they are, should presumably occur at a point where the average velocity of the heavenly bodies is small compared with the velocity of light—and this is found to be the case. Indeed the observed distribution of stellar velocities may constitute an additional confirmation of the radiation theory, though a minor and doubtful one. The average velocity of the stars is about 21 kilometers per second,[23] which is approximately seven-one hundred thousandths the velocity of light. And even the speediest known star, the rare freak of stellar velocity, R. Z. Cephei, attains a speed less than fifty times this. To obtain a rough visual estimate of this remarkable distribution of stellar velocities, imagine a straight stretch of rope a mile long attached to a stake as origin, and marked off in feet; and suppose the mile length to represent the velocity of light. Then if we were to plot stellar velocities along this rope, we should find that the average velocity would be plotted about five inches from the stake, and that some ninety per cent. of all velocities would be plotted in the first two feet, the rest scattering out more and more thinly to a maximum distance of about sixteen feet. This "striking peculiarity of the stellar system," [24] as Schlick calls it, is probably not due to accident. At any rate, whether due to the causes suggested or not, it is a peculiarity apparently, though not certainly, called for by the radiation theory and found as called for.

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[23] Eddington, A. S., *Proc. Roy. Soc.*, 1926, p. 426.

[24] Schlick, M., "Space and Time," p. 72.

Turning now from these objections and the mode of meeting them, let us proceed to the consideration of other evidence bearing on our assumptions about the constitution of the ether and its offspring matter.

What for instance can the Michelson-Morley experiment tell us about the subject under discussion, if anything. It certainly is an experiment which gives us information about the effect of motion on matter, and thus bears an analogy to the Kaufmann experiment. I shall not here describe this well known experiment, but shall content myself with reminding you that it has led to the conclusion that motion causes material bodies to shorten in the direction of their motion, but not in directions at right angles to their motion. This of course is suggestive of a Doppler effect, and this suggestiveness is emphasized when we realize that when a body moves parallel to its own length relative to the ether with velocity v, it shortens in the proportion $1 : \sqrt{1 - \frac{v^2}{c^2}}$. This is called the Lorentz contraction, and it is interesting to note that its magnitude is expressed by the reciprocal of the increase in inertia of the same body, as shown by Kaufmann's experiment, which increase we have given good reasons for believing is due to a Doppler effect. Unless all our experience of causation is misleading, this concomitance shows something common to the causes of these two changes of magnitude, and it may be worth while to inquire whether our theory of the constitution of matter sheds any light on the nature of this common something.

Note to begin with that the Michelson-Morley experiment tells us something about the effect of motion on the *shape* of a material particle. A sphere for instance would by motion be caused to become an ellipsoid of revolution with its longer axis perpendicular to the direction of motion. The corresponding experiment by Kaufmann, however, told us nothing about either the shape or the size of material particles. It only told us of their change of inertia. No particular *boundaries* to the particle are indicated. Indeed Larmor in calculating the theoretical change of inertia assumed for convenience a boundary at a great distance from the radiating center. The boundary indeed is somewhat arbitrary, since it is not the inertia, but the change of inertia, which Kaufmann's experiment reveals. As long as the boundary in the stationary and moving states is "within any regions that correspond" as Larmor expresses it, the change will come out the same. But the "particle" in its capacity as something having a boundary recognizable by its reaction upon other particles is a different matter, and it is this upon which the Michelson-Morley experiment is adapted to shed light. What I mean will become clearer if we inquire how we usually determine where the boundary or surface of a material body is. Suppose for instance we wish to measure the diameter of a steel ball with a pair of calipers. We screw the jaws of the calipers together until they come in contact with opposite surfaces of the ball, and we know when they come in contact by the resistance encountered. In short, it is the impenetrability of matter that determines boundaries, not the inertia. It is a question of how the material particle reacts to exclude other particles from the neighborhood of its center. What determines this is at present unknown. Impenetrability is one of the great mysteries of matter. Now the Michelson-Morley experiment tells us something about the change of diameter of material bodies in different directions due to their motion only. But what do we mean by the diameter of a body in any direction? How, on our assumption, shall we fix it? How much of the volume of radiation emanating from a given center of inertial mass shall be considered as comprising the "particle" in its capacity of an aggregate of energy having a boundary recognizable by its impenetrability? Where does its sphere of influence leave off? This can only be determined by experiments which involve impenetrability, and these show that, for such bodies as electrons at least, it is a very small volume indeed. The diameter of an electron has been found by experiment to be about 4×10^{-13} centimeters. Lorentz tells us, however, that by motion the longitudinal diameter is caused to contract according to a certain law. But the diameter of what? The diameter of the same amount of energy as that assumed as constituting the bounded particle when stationary? Or a different amount? Remember the energy changes when the body departs from rest relative to the ether. The succeeding spherical wave-fronts are no longer concentric. They become one-sided owing to the Doppler effect. We are forced here to make an assumption about what is *meant* by the "same particle" considered as a volume of energy having a boundary which can be fixed by its impenetrability. And what assumption is most reasonable? Shall we make the simplest assumption possible? Shall we assume that the same particle means the same amount of energy? If not what shall we mean by the same particle when the energy of the radiating center varies according to Larmor's law? What particular aggregate of energy shall we identify as the bounded or "impenetrability" particle when the particle is in motion? The least arbitrary assumption at any rate would seem to be that, as matter, by general agreement among physicists, consists entirely of energy, the same particle, so far as the penetrability of other particles is involved, means the same energy. And as this simplest and least arbitrary assumption enables us to predict the facts of nature, it would seem, on all grounds at present available, the most reasonable assumption to make. But on this assumption, what is going to happen to a particle if the energy in its vicinity is increased by motion along the straight line $x—x$ relative to the field of radiation which constitutes the ether? Evidently its volume must contract, since the energy of which it is the center has increased in intensity; that is, motion has caused energy to concentrate at the point where it is located. But as the increase is caused by a Doppler effect due to the motion, the contraction in volume will not be equal in all directions, because energy will be flowing in and out asymmetrically. And according to what law of asymmetry? This is what Larmor's law tells us. Thus if E_0 is the amount of energy in the particle when stationary relative to the ether, and D_0 its diameter, and if, in addition, the same amount of energy means the same particle, then if E_v is the energy at velocity v on the assumption of an "inertial" diameter of D_0, and D_v the impenetrability diameter, we shall have:

$$E_vD_v = E_0D_0,$$

or as

$$E_v = E_0 \frac{1}{\sqrt{1 - \frac{v^2}{c^2}}}$$

we have

$$D_v = D_0 \sqrt{1 - \frac{v^2}{c^2}},$$

which is the formula for the Lorentz contraction when D_v is the diameter along the path of motion $x—x$. Along the diameter at right angles to this, however, $D_v = D_0$, since in this direction, there being no Doppler effect, there is no concentration of energy, and along intermediate diameters will be obtained intermediate lengths according to the angle which the direction of motion of the particle makes with that of the radiation. This gives us the same law of contraction calculated by Lorentz from different assumptions, and hence explains the null result of the Michelson-Morley experiment on the simple assumption that the same aggregate of impenetrability consists of the same amount of radiant energy, whether stationary or moving, the unsymmetrical contraction consequent upon motion being due to the relation between matter and ether expressed by Larmor's law. The Michelson-Morley experiment tells the same story therefore as that of Kaufmann. And that both follow so naturally from the simplest assumptions affords evidence that our interpretation of their common origin is a plausible one, since the complete and quantitative concomitance of the laws expressive of the increase of energy and decrease of longitudinal diameter, respectively, points unerringly to a common cause.

And this leads us to an interesting presumption about the inertia of a particle and its relation to the impenetrability thereof. The inertia it would seem is not proportional to the energy contained within the particle as we have just defined it. It is proportional to the energy within a volume equal to that of the (impenetrability) particle when stationary. That is, to $\frac{1}{6}\pi D_0^3$. This is why we have, somewhat arbitrarily, referred to D_0 as the inertial diameter. It is simply the *least* arbitrary inertial diameter, but coincides with the impenetrability diameter only when the particle is stationary.

Thus when we speak of the inertia of a "particle" we are speaking of a different energy and a different volume from that to which we refer when we speak of "its" impenetrability. In its capacity as an occupier of space, the moving particle is constant in energy, but continually decreases in volume with increasing velocity. In its capacity as a resistance to change of motion, it is constant in volume, but continually increases in energy. And from Larmor's law we obtain the quantitative expression of the changes of the "particle" in both capacities. At least this appears to be the most plausible interpretation of the facts disclosed by the Kaufmann and Michelson-Morley experiments.

This interpretation may also be the answer to the second objection involved in Darwin's description of the inadequacy of the wave mechanics theory. It will be recalled that the retention of their form by fossils was cited by him as a proof that particles, though composed of mechanical waves, do not spread through space as the theory requires. But fossils and all other material bodies appear to be composed of two kinds of particles, or, to use the language of the quantum theory, of two "aspects" of a particle. The inertial aspect determines the substance or amount of mass in a particle. The impenetrability aspect determines its form. Thus the forms of fossils or other bodies do not change by virtue of the mere lapse of time, since the form is determined by the impenetrability particles of which they are composed. The inertial particles, however, are continually spreading out into space as the theory requires, the energy therefor being continually supplied through absorption of radiation from the radiant ether, just as the molecules of air in a room at constant temperature continually absorb and re-radiate the radiant heat which pervades the room. In an analogous manner the bodies of men and other animals retain their essential form, although the matter of which they are composed is continually changing through the years. Thus the life of all material particles, like the life of man, is maintained by a continual interchange of energy (and mass is a form of energy) with the environment. While present knowledge does not permit us to follow this process into its details, or to regard it as more than an hypothesis, the radiation theory, if true, renders it a possibility, and the result of the Michelson-Morley experiment renders it a plausibility. Nor has any other hypothesis been proposed which would meet the inadequacies of the wave mechanics theory cited by Darwin. At least we appear to have his word for this.

From the assumptions of the radiation theory therefore we may predict that inequality of acceleration in the impact of two bodies will be observable, that inertia and gravitation will be proportional to one another, that the heavenly bodies will move unretarded through space, that the inertia of all bodies will increase with velocity in accordance with Larmor's law and their configuration be subject to the law of the Lorentz contraction, that the velocity of light will be a maximum velocity for material bodies, that gravitation will move with this velocity and that its intensity will vary directly as the mass of bodies and inversely as the square of the distance between them—and all of these predictions are confirmed by the facts as at present understood. Furthermore, these assumptions afford more or less plausible clues to the distribution of stellar velocities, the source of stellar energy and the finite sphere of influence of gravitation. And lastly they are in entire agreement, if not indeed required by, the view of the constitution of matter maintained by the wave mechanics theory and the confirmatory experiments of Davisson, Germer and Thomson.

If time permitted, further evidence that the ether consists of a field of radiation of which matter is a modification could be presented, but time does not permit. Perhaps, however, enough has been presented to indicate the apparent constitution of the ether. It, like matter, is presumably composed of radiation. Moreover, such a presumption—unless contradicted by evidence unknown to me—is of no small interest to science. In science evidence is all that is worth appealing to, and unless I am unreasonably deceiving myself, I have in this presentation appealed to nothing else.

THE RITTENHOUSE DIFFRACTION GRATING.*

BY

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University of Pennsylvania.

On the sixteenth of March, 1785, Francis Hopkinson wrote David Rittenhouse a letter [1] which reads as follows:

"PHILADELPHIA, March 16th, 1785.[2]

Dear Sir,

I take the liberty of requesting your attention to the following problem in *optics*. It is I believe entirely new, and the solution will afford amusement to you and instruction to me.

Setting at my door one evening last summer, I took a silk handkerchief out of my pocket, and stretching a portion of it tight between my two hands, I held it up before my face and viewed, through the handkerchief, one of the street lamps which was about one hundred yards distant; expecting to see the threads of the handkerchief much magnified. Agreeably to my expectation I observed the silk threads magnified to the size of very coarse wires; but was much surprised to find that, although I moved the handkerchief to the right and left before my eyes, the dark bars did not seem to move at all, but remained permanent before the eye. If the dark bars were occasioned by the interposition of the magnified threads between the eye and the flame of the lamp, I should have supposed that they would move and succeed each other, as the threads were made to move and pass in succession before the eye; but the fact was otherwise.

To account for this phenomenon exceeds my skill in *optics*. You will be so good as to try the experiment, and if you find the case truly stated, as I doubt not you will,

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* Read before the Rittenhouse Astronomical Society during the David Rittenhouse Bicentennial Celebration, April 9, 1932.

[1] *Transactions, American Philosophical Society*, vol. 2. The writer thanks Prof. M. J. Babb for bringing the letter and the reply to his attention.

[2] Read Feb. 17, 1786.

I shall be much obliged by a solution on philosophical principles. I am sir, with great sincerity,

Your most effectionate friend,
And very humble servant,
F. HOPKINSON."

The reply[1] was read before the American Philosophical Society eleven months later, on February 17th, 1786. It begins:

"*Dear Sir:*—The experiment you mention, with a silk handkerchief and the distant flame of a lamp, is much more curious than one would at first imagine. For the object we see is not the web of the handkerchief magnified, but something very different, as appears from the following considerations."

There follow reasonings to show that Mr. Hopkinson had seen a phenomenon of the inflection of light, or as we say today, of diffraction. The reply then continues:

"In order to make my experiments with more accuracy, I made a square of parallel hairs about half an inch each way. And to have them nearly parallel and equidistant, I got a watchmaker to cut a very fine screw on two pieces of small brass wire. In the threads of these screws, 106 of which made one inch, the hairs were laid 50 or 60 in number. Looking through these hairs at a small opening in the window shutter of a dark room, 1/30 of an inch wide and three inches long, holding the hairs parallel to the slit, and looking toward the sky, I saw three parallel lines, almost equal in brightness, and on each side four or five others much fainter and growing more faint, coloured and indistinct, the farther they were from the middle line, which I knew to be formed by such rays as pass between the hairs uninfluenced by them. Thinking my apparatus not so perfect as it might be, I took out the hairs and put in others, something thicker, of these 190 made one inch, and therefore the spaces between them were about the 1/250 part of an inch. The three middle lines of light were now not so bright as they had been before, but the others were stronger and more distinct, and I could count six on each side of the middle line, seeming to be equally distant from each other, estimating the distance from the centre of one to the centre of the next. The middle line was still well defined and colourless, the next two were likewise pretty well defined, but something broader, having their inner edges tinged with blue and their outer edges with red. The others were more indistinct, and consisted each of the prismatic colours, in the same order, which by spreading more and more, seemed to touch each other at the fifth or sixth line, but those nearest the middle were separated from each other by very dark lines, much broader than the bright lines.

"Finding the beam of light which came through the window shutter divided into so many distinct pencils, I was desirous of knowing the angles which they made with each other. For this purpose I made use of a small prismatic telescope and micrometer, with which I was favoured by Dr. Franklin. I fastened the frame of parallel hairs before the object glass, so as to cover its aperture entirely. Then looking through the telescope, I measured the space between the two first side lines, and found the angular distance between their inner edges to be 13′, 15″; from the middle of one to the middle of the other 15′, 30″, and from the outer edge of one, to the outer edge of the other 17′, 45″. In the first case I had a fine blue streak in the middle of the object, and in the last a red streak. The other lines were too faint, when seen through the telescope, to measure the angles they subtended with accuracy, but from such trials as I made I am satisfied that from the second line on one side to the second on the other side, and so on, they were double, triple, quadruple, &c. of the first angles.

"It appears then that a very considerable portion of the beam of light passed between the hairs, without being at all bent out of its first course; that another smaller portion was bent at a medium about 7′, 45″ each way; the red rays a little more, and the blue rays a little less; another still smaller portion 15′, 30″; another 23′, 15″, and so on. But that no light, or next to none, was bent in any angle less than 6′, nor any light of any particular colour, in any intermediate angle between those which arise from doubling, tripling, &c. of the angle in which it is bent in the first side lines.

"I was surprized to find that the red rays are more bent out of their first direction, and the blue rays less; as if the hairs acted with more force on the red than on the blue rays, contrary to what happens by refraction, when light passes obliquely through the common surface of two different mediums. It is, however, consonant to what Sir Isaac Newton observes with respect to the fringes that border the shadows of hairs and other bodies; his words are, 'And therefore the hair in causing these fringes, acted alike upon the red light or least refrangible rays at a greater distance, and upon the violet or most refrangible rays at a less distance, and by those actions disposed the red light into larger fringes, and the violet into smaller fringes.'

"By pursuing these experiments it is probable that new and interesting discoveries may be made, respecting the properties of this wonderful substance, light, which animates all nature in the eyes of man, and perhaps above all things disposes him to acknowledge the Creater's bounty. But want of leisure obliges me to quit the subject for the present.

"I am, dear sir, your affectionate servant,
"And very humble servant,
"DAVID RITTENHOUSE."

Those familiar with optics recognize in this reply the description of a plane diffraction grating of the transmission type. The array of apparatus is one now known wherever optics is studied. The source of light is a narrow illuminated slit. It is viewed through the grating, which is held normal to the incident light, with its lines parallel to the slit.

On either side of the slit is seen an array of spectra. Rittenhouse saw six orders. He observed that red is deviated most, blue least, "contrary to what happens by refraction." The increase of dispersion and the decrease of brightness as the order increases are recorded.

He reports a law of distribution of these spectra in space. The angle between the center of the *n*th order on the right, and the center of the same order on the left is *n* times the corresponding angle for the first order. This law is a corollary of a more general law discovered thirty years later by Fresnel and still later independently by Fraunhofer. It is valid as a corollary when the deviations from the original path of the incident light are small.

The dimensions of his grating and the angles measured by Rittenhouse permit one to check his work. From them I have calculated a wave-length of 6200 Angstroms for red, and 4600 for blue. Both are reasonable mean values, and give evidence that the observations were made with care.

To appreciate the achievement one must see it in its historical setting. A great age in optics ended just before David Rittenhouse was born. In the century preceding his birth there lived and died Grimaldi, Bartholinus, Boyle, Huyghens, Hooke, and Newton. The last and greatest of them, Sir Isaac Newton, died in 1727. He had adopted a corpuscular theory of light, and for the rest of the eighteenth century men thought in terms of it. The rival theory of Huyghens, that light consists of waves in an all-pervading ether, was rejected as incompetent to account for the propagation of light in straight lines.

In 1786, the year of Rittenhouse's experiment, another great era was about to begin. Many of its great figures were already born. Thomas Young was thirteen years old; Biot was twelve; Malus eleven, and David Brewster five. Arago, Fraunhofer, and Fresnel were born during the next two years.

In 1801 Dr. Thomas Young revived the wave-theory of light and added to it the principle of interference. In 1815 Fresnel independently did the same things and in a memoir[3] on diffraction developed the general law of the diffraction grating. His editor, Verdet, says:[3] "The formulas established by Fresnel . . . express exactly the laws of grating colors which Fraunhofer discovered by observation several years later." In 1821 Fraunhofer made gratings and with them measured the wave-lengths of the solar lines which bear his name.

In histories of optics one reads that Joseph Fraunhofer was the first man to use gratings and to observe diffraction spectra through them. As instances:

Cajori's "History of Physics" in speaking of Fraunhofer, says: "He was the first to observe spectra due to gratings."

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[3] Works of Fresnel, Vol. 1, pp. 45–51.

The Encyclopedia Brittanica, 14th Edition, in an article on "Spectroscopy" by Fowler, says: "Gratings were first used by Fraunhöfer about 1820."

Crew's "Rise of Modern Physics" says: "It was about this time (1819) that Biot and Fresnel were making clear the phenomena of diffraction; and upon this basis Fraunhofer proceeded to manufacture a great variety of plane gratings, wire wound on screws, etc."

It has been my privilege to lay before you the evidence that in 1786 David Rittenhouse made a diffraction grating. Through it he observed spectra. He described them as we now know them. He discovered a corollary of the general grating law. He left a record of observations and measurements which is intelligible today. Beyond this he did not go, for he lacked leisure to continue his experiments, and besides the fertile ideas of the undulatory theory and of the principle of interference were not yet current. In a few years they became current and opened a new world of investigation.

Nevertheless Rittenhouse foresaw and foretold that "by pursuing these experiments new and interesting discoveries may be made, respecting the properties of this wonderful substance, light." The history of optics during the nineteenth century is a fulfillment of this prophecy.

QUANTUM PHENOMENA IN THE BIOLOGICAL EFFECTS OF RADIANT ENERGY.

BY

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Considerable attention has been paid recently to the possibility that the quantum nature of the light absorption process may play an important role in the mechanism whereby unicellular or other simple organisms are killed by ultra-violet and x-radiation. Some interesting experimental results in this direction have just been reported by Swann and del Rosario[1] from the Bartol Research Foundation and by others whose work is cited in their paper.

But there has been, it seems to me, an unfortunate neglect of the proper literature citations in regard to the theoretical ideas in this field. Actually although a footnote in the paper just cited says that "the senior author . . . is also responsible for the theoretical treatment," the credit for first working out these ideas seems to be due to Crowther.[2] He developed the essential details of the picture as used by later workers and applied it to some observations on the killing of *Colpidium Colpoda* by x-rays. Here the experiments, interpreted by the statistical theory, indicated that a large number of quanta (about 50) need to be absorbed by one organism to cause death, so the quantum nature of the phenomena is not much in evidence.

Independently in 1927 the writer and H. M. Terrill developed the same theory and applied it to data secured by Packard in the cancer research institute of Columbia University on the killing of *Drosophila* eggs by x-rays. The paper[3] appeared in the spring of 1928 although the journal was dated

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[1] Swann and del Rosario, JOURNAL OF THE FRANKLIN INSTITUTE, **213**, 549 (1932).

[2] Crowther, *Proc. Roy. Soc.*, **B 100**, 390 (1926).

[3] Condon and Terrill, *Journal of Cancer Research*, **11**, 324 (1927).

December 1927. After completing our work we discovered the paper of Crowther but did not hesitate to acknowledge his priority in the matter. Perhaps it is not inappropriate here to enter a protest against an editorial high-handedness which our paper experienced. Our manuscript read: "It is a well-known fact of experimental physics . . ." and the word "fact" was changed to "theory" without securing the consent of the authors, a consent which of course could not have been obtained if they had been consulted.

The next work in this field besides that of Gates[4] which is cited by Swann and del Rosario is that of Holweck,[5] Lacassagne,[6] and Mme. P. Curie,[7] all published in January 1929. The first two are experimental and give adequate reference to the earlier work. The latter again derives the same theory, without reference to previous workers, although she does advance matters by some interesting observations on the nature of the theoretical curves.

Next, in 1930, 1931, and 1932, we find a series of papers by Wyckoff and co-workers at the Rockefeller Institute in New York, some of which are cited by Swann and del Rosario, and which are also characterized by a complete new restatement of the same theory without allusion to the previous theoretical workers in the field.

It is not my desire in this note to advance any petty claims to priority for Crowther or myself. I simply feel that scientific research, being a social enterprise, demands that adequate attention be paid to literature citations both as to accuracy and as to completeness.

Editor's Note.—In the article by Swann and del Rosario, to which reference is made (Vol. 213, p. 550, May 1932) by Prof. Condon, the authors included the following statement:

" . . . The present paper is an elaboration of the results already published by the authors upon this subject. . . . No particular claim is made as to originality of procedure in the matter of questions investigated, and the processes adopted are such as would be natural to any experienced worker in the field. The points of view elaborated have doubtless occurred to many who have thought about the question, or have investigated it. . . ."

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[4] Gates, *Jour. Gen. Physiol.*, **13**, 231, 249 (1929).

[5] Holweck, *Comptes Rendus*, **188**, 197 (1929).

[6] Lacassagne, ibid., **188**, 200 (1929).

[7] Curie, ibid., **188**, 202 (1929).

NOTES FROM THE U. S. BUREAU OF STANDARDS.*

AIRPLANE RUDDERS.

The behavior of an airplane with respect to recovery from a spin has a most important bearing on its safety. The Air Commerce Regulations have rather stringent requirements as to the ease and quickness of recovery, requiring recovery in 1½ turns with controls neutral. The studies of recovery from a spin carried out in this country and abroad have indicated that the most useful control in stopping the spin is the rudder. Hence a knowledge of the effectiveness of rudders as dependent on various design factors is most desirable.

Previous studies of the effectiveness of rudders have usually been made in connection with the design of a particular airplane. By the method of trial and error, an arrangement of the vertical surfaces is found which gives a satisfactory yawing moment for a given angular displacement of the rudder as judged by comparison with measurements on models of airplanes whose rudder control is known to be satisfactory. Such measurements do not readily lend themselves to analysis or to the determination of the influence of the several factors, such as the area and aspect ratio of the vertical tail surfaces, on the magnitude of the yawing moments. An investigation recently conducted at the bureau represents a beginning at least of a systematic study of the effect of the area and the aspect ratio of the vertical surfaces, of the angle of pitch, and of the shape of the fuselage on the yawing moments produced by rudder displacement.

The effectiveness of a rudder is approximately proportional to its angular displacement for angles less than 25°. The effectiveness continues to increase with increasing rudder angle at approximately the same rate for rudders of small aspect ratio, but for rudders of large aspect ratio, the rate of increase falls off rapidly above rudder angles of 25°. The value of the rudder moment, however, is never less than for corresponding rudders of small aspect ratio. The term "aspect ratio" means roughly the ratio of the width of the vertical tail surface measured perpendicular to the airplane fuselage to the length measured along the fuselage.

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* Communicated by the Director.

The effect of increasing the angle of pitch is to decrease greatly the control at a given rudder angle; the decrease being greatest for rudders of large aspect ratio, when the rudder angle is less than 25°; but when the rudder angle is large (44°), the decrease is greatest for rudders of small aspect ratio.

The effect of the shape of the fuselage is quite noticeable, being especially marked in the case of the cabin fuselage. The shielding effects are such for the cabin fuselage that the effectiveness at a given rudder setting at an angle of pitch of 40° is about two-thirds of that for the open cockpit fuselages.

When the aspect ratio is maintained constant, the effectiveness of the rudder is linearly related to the area ratio, but increases somewhat faster than in direct proportion.

The effect of aspect ratio is sufficiently large to be considered in design. If rudder angles approaching 45° are permitted, the effect of increasing the aspect ratio is small and may be ignored for practical purposes, although at high angles of pitch, large aspect ratio is appreciably favorable. If the rudder angle is restricted to 25° or less, an increase of rudder control of 30 to 45 per cent. may be produced by increasing the aspect ratio from 1 to 2.

COMBUSTION PROCESSES IN AIRCRAFT ENGINES.

In aircraft, to secure the most horsepower per pound of engine weight and maximum cruising range without refueling it is necessary to use supercharged engines of high compression ratio. Such engines will overheat and may suffer serious mechanical damage if the combustion phenomena known as preignition and detonation occur. Preignition commonly arises from overheated spark plugs or inadequately cooled exhaust valves and shows itself by loss of power and a tendency to back-fire. Detonation is what the motorist calls "knock" and commonly results from using unsuitable fuel. Continued detonation will heat the cylinder head until preignition sets in.

Experiments on combustion in engines, made in various laboratories for the purpose of learning more about the nature of detonation, were discussed by H. K. Cummings of the bureau at a recent aeronautical meeting of the American Society of Mechanical Engineers in Buffalo. These experiments indicate that a narrow combustion zone proceeds from the spark plug at a rate depending on engine speed and carburetor adjustment and that detonation usually does not occur until this burning zone has nearly crossed the combustion chamber. In the event of detonation the remaining portion of the charge, which is already heated and highly compressed, burns with extreme rapidity. This rapid burning of the last portion of the explosive charge probably is due to compression and temperature rise and to chemical actions which have taken place during the compression of the charge. The only known effect that adding tetraethyl lead to a gasoline has on combustion is to prevent the ultra-rapid burning of a portion of the charge and the accompanying abrupt pressure rise which constitute the knock. Precisely how this is accomplished has not been established as yet.

ANALYTICAL METHODS FOR PHOTOGRAPHIC EMULSIONS.

The sensitivity of photographic emulsions depends to a large extent on the presence of nuclei on the grains of silver bromide. These nuclei consist of silver in other forms; silver sulphide is known to be present, and metallic silver is quite possible. At the bureau a critical study has been made of the analytical methods proposed for the determination of the silver present in these forms in an emulsion. It is found that reliable results can be obtained by the method of Weigert and Lühr, which consists of a double fixation with sodium thiosulphate ("hypo") and extensive washing, after which the silver remaining is determined by electrometric titration. The tests of the method go considerably further than those of the originators. The results at the bureau confirm the unexpected discovery of the German investigators that unexposed emulsions contain silver or silver sulphide in quantities greater than that involved in the formation of the latent image.

Another important constituent of emulsions is the trace of soluble bromide which acts as a preservative. Determination of soluble bromide based on simple extraction with water may give results that are much too high because of the formation of silver-gelatin compounds (report of which was published in B. S. Research Paper No. 376) and consequent decomposition of the silver bromide. This error may be avoided by extracting the bromide with dilute acid.

The complete discussion of this work will be published as Research Paper No. 447 in the June number of the *Bureau of Standards Journal of Research.*

A STANDARD ULTRA-VIOLET DOSAGE INTENSITY METER.

During the past two years an investigation has been in progress at the bureau on various methods of measuring ultra-violet radiation useful for healing purposes. The demand for such information arises from the fact that numerous kinds and varieties of ultra-violet light sources are being offered to physicians and to the public, and practically no exact data are available regarding their effective ultra-violet output. This is partly owing to the fact that there are no intensity (dosage) meters available for accurately evaluating the ultra-violet emitted.

Assuming that the lamps emit sufficient ultra-violet to produce a therapeutic effect, it is important to determine the amount that can be applied without burning the patient. This depends upon the tolerance of the skin, the susceptibility of which varies greatly for different persons, and even for the same person, depending upon moisture and temperature conditions of the atmosphere.

In the present investigation which will be fully described in Research Paper No. 450 in the June number of the *Bureau of Standards Journal of Research,* the amount of ultraviolet that can be safely applied to the skin, which is determined by the erythema produced, is correlated with the energy measurements. The experimental procedure consisted in comparing the erythema produced (on, say, the inner forearm) by a measured amount of homogeneous radiation of the wave-length 297 millimicrons, used as a standard, with the erythema produced by a measured amount of heterogeneous ultra-violet radiation, from various sources, evaluated by means of the balanced thermocouple and filter radiometer.

A very close agreement was found between the erythemal and the radiometric evaluation of the ultra-violet emitted by various sources which differed very markedly in ultra-violet spectral energy distribution; including, as extremes, the sun, which has practically no ultra-violet radiation of wave-lengths less than about 290 millimicrons, and the so-called "cold quartz" mercury vapor lamp in which over 95 per cent. of the total erythemogenic ultra-violet radiation is contained in the resonance line at 254 millimicrons. Other sources examined were the Mazda S-1 and S-2 mercury arc lamps in Corex D bulbs, the high temperature quartz mercury arc lamp, and the carbon arc lamp with "therapeutic B, C, and white flame" electrodes.

Based upon a general survey of the biological and radiometric data available, the conclusion reached is that, in order to insure effective therapeutic results, the radiant flux from the source of heterogeneous ultraviolet radiation of wave-lengths less than and including 313 millimicrons, evaluated according to the standard spectral erythemic response curve, should not be less than the erythemogenic equivalent of 20 microwatts per square centimeter of homogeneous radiation of the wave-length of maximum erythemic effectiveness, which wave-length, for practical purposes, is taken at the emission line of mercury, at 297 millimicrons (2967A).

For the various sources of heterogeneous ultra-violet radiation of wave-lengths less than and including 313 millimicrons, the erythemogenic equivalent of 20 microwatts per square centimeter of homogeneous radiation of wave-length 297 millimicrons, in microwatts per square centimeter, is approximately as follows:

Sun	91
Mazda S-1 lamp	80
Mazda S-2 lamp	93
Quartz mercury arc	58
So-called "cold quartz" mercury vapor lamp	36
Carbon arc	45 to 90
Mazda CX tungsten filament	130

In this paper the unit of dosage is discussed, and 10,000 ergs per square centimeter of homogeneous radiation of the mercury arc at the wave-length of 297 millimicrons is suggested for the erythemogenic unit of dosage.

MEASUREMENT OF THE pH OF RUBBER LATEX BY THE ANTIMONY ELECTRODE.

The antimony electrode is being investigated at the bureau, as a means of determining the pH of the ordinary ammonia-preserved rubber latex. Measurements that have been made on latex samples ranging from pH 8 to pH 11 indicate that this electrode gives constant and reproducible readings and is free from disadvantages inherent in other types of electrodes when used in latex. The hydrogen electrode cannot be employed in latex on account of the deposition of rubber on the platinum black surface. The quinhydrone electrode gives erroneous results when the latex is in the strongly alkaline range above, say, pH 8.5. The glass electrode, on the other hand, has given good results with latex, but has the practical disadvantage that either an electrometer or a vacuum tube galvanometer is required to measure the electromotive forces through the high resistances that are involved. The antimony electrode, however, can be used with ordinary electrometric apparatus which is relatively simple in operation.

The antimony electrode is used in the form of a cast stick of the chemically pure metal, and is calibrated in appropriate buffer solutions against the hydrogen electrode.

This work is being done in connection with the development of a general procedure for making laboratory test specimens directly from rubber latex by electrodeposition.

DETERIORATION OF PAPER IN ACID-POLLUTED AIR.

The effect of small amounts of sulphur dioxide upon current commercial book and writing papers has been studied by the bureau, and the results are now available in printed form. Fourteen different commercial book and writing papers were exposed for 10 days to the action of air containing 2 to 9 parts of sulphur dioxide per million. At the end of that time it was found that the papers had suffered a considerable decrease in folding strength, 40 per cent. in some cases, and had undergone changes in chemical characteristics.

The effect of sulphur dioxide, released into the atmosphere by the combustion of modern fuels, has been the subject of many investigations. (Technical News Bulletin No. 177, p. 4; January, 1932.) A study of statistics obtained from reports of these investigations, covering 15 American and European cities, showed air pollutions ranging from 0.20 to 3.00 parts sulphur dioxide per million and an annual precipitation of sulphuric acid ranging from 11 to 190 tons per square mile. Additional evidence of the increasingly polluted nature of modern urban atmospheres resulted from a recent investigation by the bureau of the causes of the so-called "winter damage" or rotting of damp cotton fabric upon exposure to outside air, experienced by laundries throughout New England (Technical News Bulletin No. 167, p. 31; March, 1931). Atmospheric sulphur dioxide, which is oxidized on the fabric to sulphuric acid, was found to be the cause of the phenomenon.

The initial quality of the papers was apparently no measure of their resistance to the action of sulphur dioxide, since some of the low-grade papers, unsuitable for permanent record use, as determined by the usual tests, suffered less from exposure to this gas than did high-grade papers. This fact adds to the evidence in favor of the modification of library ventilating systems so as to eliminate from the library air, as far as practicable, any acid pollution introduced by the admission of outside air.

The research reported is one phase of a general study of library storage of records being made at the bureau with the assistance of a fund granted for the purpose to the National Research Council by the Carnegie Corporation. The report is contained in Research Paper No. 407, entitled "The Deteriorative Effect of Sulphur Dioxide upon Paper in an Atmosphere of Constant Humidity and Temperature." Copies may be obtained from the Superintendent of Documents, Washington, D. C.

GLASS PHASE IN FIRED CLAY MATERIALS.

The strength and durability of ceramic products depend partly on the formation during the firing process of a glass phase which binds together the mineral grains of the material. Only in rare cases does the durability of the material depend alone on the presence of interlocking or fitted crystals with no glassy bond. The properties of fired clay wares become more dependent upon their glass phase the nearer they approach complete vitrification in the kiln. In extreme cases, such as completely vitrified ware and glass ware, obviously the properties of the glass phase entirely determine the properties of the ware.

Because of the importance of this matter a study is being made to determine the nature of the glass phase in fired clay materials, physical measurements being made on annealed glasses whose compositions are known. Three methods used in this investigation are: (1) to fuse to a homogeneous glass the material such as an analyzed china clay and determine the physical properties of the resulting glass; (2) to fuse to a glass those eutectics which possibly occur in the clay and make the physical measurements of the glasses; and (3) to study the glasses resulting from fusion of mixtures of definite composition in a four component system.

In the selection of raw materials, mixing of the weighed batches and fusion to a glass, great care is taken to prevent the introduction of impurities. Whenever possible the fusion is conducted in platinum. A sufficient number of fusions, with fine grinding after each, are made until microscopic examination shows that the resulting glasses are homogeneous. The sample of glass is then annealed and the physical measurements made on it. These measurements include coefficient of expansion and softening point, Young's modulus of elasticity, density and specific volume, index of refraction, and tensile strength. Studies have been made on glasses resulting from fusion of five pure china clays: five eutectics in the K_2O-CaO-SiO_2 system; and a portion of the tetrahedron rich in silica in the four component system K_2O-CaO-Al_2O_3-SiO_2. Tests have also been made on blends of these glasses with muscovite, and with the eutectics between anorthite, mullite and SiO_2.

USE OF BRINELL TEST ON KEENE'S CEMENT.

Keene's cement, which is a type of dead burned gypsum plaster, is commonly regarded as possessing certain advantages over ordinary plaster of Paris for plaster work. Of these advantages, the main one is that of hardness of surface. Therefore, when the properties of Keene's cement were being investigated with a view towards formulating Federal specifications, it was suggested that the Brinell hardness test, such as is used for testing metals, be tried on castings made from the various types of Keene's cement, as well as the ordinary calcined gypsum (plaster of Paris).

Tests were made on two inch cubes made from nine different Keene's cements. Three consistencies of each material were used. A few tests were also made on cubes cast from plaster of Paris. A pressure of 500 kg. was applied for one minute through a steel ball 17.5 mm. in diameter on to the surfaces of the cubes. Four tests were made on each cube, one on each of the surfaces cast in contact with the mold. The diameter of the resultant indentation was measured by means of a micrometer microscope to the closest 0.1 mm. The Brinell number is the load per unit of spherical surface of contact.

In some cases, mostly among the specimens of regular plaster of Paris, the specimens broke when the load was applied. Strength tests on other cubes cast of the same materials showed that these castings had in all cases a compressive strength of 3,000 lbs./in.^{2} Of those materials which did not break, only one mix gave a strength below 3,200 lbs./in.^{2} in compression.

The indentations ranged from 6.7 to 9.7 mm. (Brinell numbers of 14.4 to 6.3) for the Keene's cements tested. The cubes made from the calcined gypsums either broke because of their lower strengths or gave indentations slightly larger than 9.7 mm. Test specimens made from a special type of dental plaster gave the low indentation of 5.8 mm.

It was found that the compressive and tensile strengths are both related to the Brinell number, the higher the strengths the larger the Brinell number or the smaller the indentation. The compressive strength of the Keene's cements ranged from 5,760 to 3,070 lbs./in.,^{2} the mixes showing these extreme values being the same as those which gave the extreme Brinell numbers. The special dental plaster showed a compressive strength of 9,300 lbs./in.^{2}

From the data obtained it would appear that the Brinell test might be better than the strength tests in testing Keene's cement. It measures more directly the property desired in the material and is also a very simple test which can be performed in a short time. The several tests on the same cube and those from other cubes of the same mix checked within 0.3 mm. in all cases.

Aluminum Conductors.—(*Power*, **75**, 685.) Electrical conductors of pure aluminum have been used in United States since about 1898. Originally used in the form of stranded cables, today aluminum conductors may be had in three major forms: flat bar, tubing and structural shapes such as channels, angles and T's. Comparisons between aluminum and copper conductors are interesting. An aluminum conductor, the direct-current resistance of which is the same as copper has a cross-sectional area of about 160 per cent. of the latter, but its weight is less than half. If a maximum temperature rise rating is considered, the aluminum cross-section need be only approximately 127 per cent. and its weight less than 40 per cent. In flat or tubular buses where short-circuit forces control the spacing of the supports, the latter may be located at least as far apart as for copper, because the required additional cross-section area makes up for the lower tensile strength of aluminum, 12,000 to 24,000 lb. per square inch, according to the temper. Where high strength is more important than conductivity, strong aluminum alloys are available with conductivities ranging from 30 to 50 per cent. of copper and tensile strengths as high as 55,000 lb. per square inch.

T. K. C.

Course in Scientific Crime Detection.—If anyone fears the source of detective-story material is rapidly diminishing let them be reassured. The News Edition of *Ind. Eng. Chem.* for May 20 states the Philadelphia College of Pharmacy and Science will offer a one-year graduate course in scientific methods of crime detection. "Beginning in September, 1932, there will be classes in finger-printing and finger-print analysis; the study of weapons, projectiles and ammunitions; various methods of collecting and recording scientific evidence; analysis and identification of foods, drugs, fibers, textiles, and miscellaneous materials; making and interpretation of maps, plans and drawings; the study of poisons and their separation and identification from food, blood, and cadaveric material; and laboratory periods devoted to training and observation, particularly emphasizing microchemistry, microphotography, and crystallography. The college is also establishing a museum, which will be open to police officials all over the world, and in which will be kept such articles as samples of every brand of cigarette manufactured anywhere in the world, patterns of tire tread of every make of automobile, every grade of paper and twine used, and analyses of different types of soil."

T. K. C.

THE FRANKLIN INSTITUTE.

COMMITTEE ON SCIENCE AND THE ARTS.

(*Abstract of Proceedings of Stated Meeting held Wednesday, June 8, 1932.*)

HALL OF THE COMMITTEE,
PHILADELPHIA, JUNE 8, 1932.

MR. EDWARD L. CLARK *in the Chair.*

The following report was presented for final action:

No. 2948: Liquid Purification Process.

This report recommended the award of the John Price Wetherill Medal to The Koppers Company, of Pittsburgh, Pennsylvania, "In consideration of the development of systems for the liquid purification of gases which have proven to be commercially successful, as evidenced by the number of such installations in regular use."

The following report was presented for first reading:

No. 2942: Automatic Control Device for Multi-Motored Airplanes.

GEO. A. HOADLEY,
Secretary.

MEMBERSHIP NOTES.

CHANGES OF ADDRESS.

MR. S. H. BUNNELL, 2245 Main Street, Stratford, Conn.
MR. W. C. CUSHING, 6607 Quincy Street, Philadelphia, Pa.
MR. L. A. FLANAGAN, Hotel Albermarle, Pacific and Virginia Avenues, Atlantic City, N. J.
MR. W. F. GRAHAM, Wynnewood, Pa.
MR. WALTER G. HENSEL, 2828 Cove Road, Merchantville, N. J.
DR. H. B. HOLROYD, Trumansburg, N. Y.
MR. C. N. JOHNSON, Westinghouse Electric and Mfg. Company, 150 Broadway, New York City.
DR. S. S. KISTLER, Chemistry Department, University of Illinois, Urbana, Illinois.
PRESIDENT RUSSELL E. LAWRENCE, Lawrence Institute of Technology, 15100 Woodward Avenue, Highland Park, Michigan.
MR. W. LAURENCE LEPAGE, B-302 Ridley Manor, 100 Morton Avenue, Ridley Park, Pa.
MR. NATHAN M. LOWER, 325 East Forty-first Street, New York City.
MR. W. C. MELCHER, JR., 56 West Willow Grove Avenue, Germantown, Philadelphia, Pa.

DR. FREDERIC PALMER, JR., Wellfleet, Mass.
DR. SAMUEL E. POND, Box 204 Woods Hole, Mass.
REAR ADMIRAL W. S. SIMS, 73 Catherine Street, Newport, R. I.
MR. J. W. VAN DYKE, The Barclay Hotel, Eighteenth and Rittenhouse Streets, Philadelphia, Pa.
MR. JOHN E. ZIMMERMANN, *President*, United Gas Improvement Company, 1401 Arch Street, Philadelphia, Pa.

NECROLOGY.

Mr. W. P. Bement, Philadelphia, Pa.
Mr. L. D. Burlingame, Providence, R. I.
Mr. C. L. Edgar, Boston, Mass.
Mr. C. E. Ingersoll, Philadelphia, Pa.

LIBRARY NOTES.

RECENT ADDITIONS.

ALBERT, C. D., AND F. S. ROGERS. Kinematics of Machinery. 1931.

ALLEN, T. Uniflow, Back-Pressure and Steam Extraction Engines.

American Gas Association. Proceedings. Thirteenth Annual Convention, 1931.

Annual Review of Biochemistry. Edited by James Murray Luck. Volume I. 1932.

BANGAY, R. D. The Elementary Principles of Wireless Telegraphy and Telephony. Third edition, revised by O. F. Brown. No date.

BOCK, GEORGE E. What Makes the Wheels Go 'Round. 1931.

BRAYMER, DANIEL H., AND A. C. ROE. Rewinding and Connecting Alternating-Current Motors. First edition. 1932.

BUNNETT, L. E. Estimating for Mechanical Engineers. A Book on the Computation of Estimates for Works Managers, Foremen, Rate-Fixers, Cost-Clerks and Students. 1932.

DINSDALE, A. First Principles of Television. 1932.

EMICH, FRIEDRICH. Microchemical Laboratory Manual. With a section on Spot Analysis by Dr. Fritz Feigl. Translated by Frank Schneider. 1932.

FRANKLIN, BENJAMIN. The sayings of Poor Richard. The Prefaces, Proverbs, and Poems of Benjamin Franklin, Originally Printed in Poor Richard's Almanacs for 1733–1758. Collected and edited by Paul Leicester Ford. No date.

GIULI, ITALO DE. Submarine Telegraphy: a Practical Manual Translated from the Italian by J. J. McKichan. 1932.

GRAY, R. WHYTLAW-, AND H. S. PATTERSON. Smoke: a Study of Aerial Disperse Systems. 1932.

HAYNES, MERRITT WAY. The Student's History of Printing, Giving the Principal Dates, Personages, and Events in the Development of the Typographic Art from Earliest Times to the Present, in Chronological Order. First edition. 1930.

Henry, Alfred. Le Calcul des Différences Finies et ses Applications; avec de Nombreux Examples Traités dans Tous leurs Détails et un Grand Nombre d'Exercices Suivis de leurs Réponses. Traduit de l'anglais par A. Sallin. 1932.

Hoopes, Penrose R. Connecticut Clockmakers of the Eighteenth Century. 1930.

Houstoun, R. A. Vision and Colour Vision. 1932.

Hoyer-Kreuter technologisches Wörterbuch-Sechste vollkommen neubearbeitete Auflage herausgegeben von Alfred Schlomann. Erster Band: Deutsch-Englisch-Französisch. 1932.

Humboldt, Alexander von. Personal Narrative of Travels to the Equinoctial Regions of America, during the Years 1799–1804 by Alexander von Humboldt and Aimé Bonpland. Written in French by Alexander von Humboldt and translated and edited by Thomasina Ross. In three volumes. 1852–1853.

Institute of British Foundrymen. Proceedings. Volume 24, 1930–1931.

Jahresbericht über die Leistungen der chemischen Technologie für das Jahr 1931. 77 Jahrgang 1 Abteilung: Unorganischer Teil. 1932.

Kassel, Louis S. The Kinetics of Homogeneous Gas Reactions. American Chemical Society Monograph Series No. 57. 1932.

Kirkham, John Edward. Highway Bridges, Design and Cost. First edition. 1932.

Laplace, P. S. de. Philosophischer Versuch über die Wahrscheinlichkeit. Herausgegeben von R. v. Mises. Ostwald's Klassiker der exakten Wissenschaften. Nr. 233. 1932.

[London] Metropolitan Water Board. Twenty-eighth Annual Report for the Year Ended 31st March, 1931.

Melluish, R. K. An Introduction to the Mathematics of Map Projections. 1931.

Parr, S. W. The Analysis of Fuel, Gas, Water and Lubricants. Fourth edition. 1932.

Peters, John P., and Donald D. Van Slyke. Quantitative Clinical Chemistry. Volume 2: Methods. 1932.

Robbins, E. C., and F. E. Folts. Industrial Management: a Case Book. First edition. 1932.

Still, Alfred. Elements of Electrical Design. Second edition. 1932.

Sugar Reference Book and Directory. 1931.

Winchell, Alexander Newton. The Microscopic Characters of Artificial Inorganic Solid Substances or Artificial Minerals. With a chapter on the Universal Stage by Richard C. Emmons. Second edition. 1931.

BOOK REVIEWS.

Introduction to the History of Science. Vol. II (in two parts): From Rabbi Ben Ezra to Roger Bacon. By George Sarton, Associate in the History of Science, Carnegie Institution of Washington. xii–1251 pages 18 x 26.6 cms. Baltimore, the Williams & Wilkins Co. Price $12.00.

It used to be the fashion for uninformed writers to sneer at the middle ages as a period lacking in intellectual accomplishment. As a reaction from this attitude there have been such eulogies of the time as that of J. J. Walsh, who called the thirteenth the "greatest of centuries." If the old belief still persists, this scholarly work by Dr. Sarton should lay it to rest. Avoiding both extremes, he shows in the two bulky volumes how "the twelfth century, and to a lesser extent the thirteenth century, was essentially a period of transition and compromise," as he states in his excellent introductory chapter. "Never were the exchanges between the three main civilizations of Europe and the Mediterranean world—the Jewish, the Christian and the Muslim—more intense than in the twelfth century." From this emerged our own European civilization, as a triumph of the West over the East, but, as the author points out, "when we speak of a western triumph, we must not think of it as if eastern ideas had been then and there superseded by western ones. The western victory consisted rather in an assimilation of the East by the West. The treasures of Greco-Arabic science were feverishly poured out from Arabic vessels into Latin and Hebrew ones. . . . Thus the western victory did not imply for mankind a change of purpose or direction, but simply a change of leadership."

In the 99 page introductory chapter he summarizes the work of the period in the sciences of mathematics, astronomy, physics, chemistry, geography, natural history and medicine. The rest of the text is divided into four books, covering, respectively, the first and second halves each of the twelfth and thirteenth centuries. Because of the bulk of the work, it is divided into two parts, Books III and IV constituting the second.

Book I is typical of the author's treatment. The first chapter is a survey, in 43 pages, of science and intellectual progress in the first half of the twelfth century. Then follow chapters on "Religious Background," "The Translators," "Philosophic Background," "Mathematics and Astronomy," "Physics, Technology and Music," "Chemistry," "Geography," "Natural History," "Medicine," "Historiography," "Law and Sociology," and "Philology and Education." Each chapter is further subdivided into a number of sections covering writers of different nations. Under these headings is a paragraph (or, in some cases, several) devoted to each author, giving a brief account of his work of importance, and a bibliography. Not only does he treat of European writers, but also those in the Orient as well, including those in China. At the conclusion is an index, a remarkable thing in itself, for it covers 107 pages. There is also an index to Greek names.

It is quite impossible to review a work such as this in the usual way, for it represents the fruit of so many years of scholarly researches, and so far surpasses anything now extant on the subject, that one can only accept it with a feeling of the deepest gratitude to Dr. Sarton. In the first volume, which appeared in 1927, he covered the period from Homer to Omar Khayyam. In the second volume, the present work, he carries his story from Rabbi Ben Ezra to Roger Bacon. One wonders how many volumes will be required before he reaches the time of, let us say, Newton. Evidently the author fully realizes the magnitude of his task, and the limitations of one lifetime, for he says in his preface, "It is already clear that I shall not be able to carry my investigations down to the twentieth century, but I hope that when the time of my release comes, my plans will be so well understood, and other scholars so well trained, that the work I began may safely be continued." Students of the history of science, indeed, of intellectual progress in general, throughout the world will hope that Prof. Sarton will be spared for many years of future work. But it is gratifying to know that its continuation is assured, and that a large amount of additional material is already rapidly being brought into shape for publication.

JAMES STOKLEY.

THE MODERN MOTOR-DRIVEN WOODWORKING SHOP. By Herbert E. Tautz and Clyde J. Fruits. Three volumes, 384 pages, illustrations, 23 x 16 cms., Milwaukee, Woodworkers' Educational Department (Division of Delta Manufacturing Company), 1930. Price $1.00 per volume.

Modern power-distribution available either in large or small amounts, has made it possible for the amateur mechanic or the small-laboratory workman to produce work rapidly and of high quality. Among the firms who supply equipment adapted to that purpose, the Delta Manufacturing Company have specialized in the production of a class of remarkably practical small woodworking power tools of excellent design and workmanship.

These volumes have been prepared to provide complete information on the installation, use, care and upkeep of these tools as well as instruction in the technique of their operation. Although the copious and finely executed collection of illustrations is of Delta tools, the methods which are set forth in the greatest detail apply generally to all tools of the type described. The first volume covers the planning of the workshop, uses of the circular saw including dado and moulding-cutter work, and an account of the construction and operation of the jointer. The second volume includes the wood-turning lathe, the band saw, the scroll saw, the boring routing and mortising machine with practical data on electric motors. The third volume deals with the construction of various articles of furniture, wood finishing and glues and their uses. It is of interest to note the sizes of the tools described. The circular saw is eight inches in diameter, the lathe is of nine inch swing, and the band saw is made with eight- and ten-inch pulleys.

The work should prove valuable for reference on modern woodworking methods as well as a reliable guide in woodshop technique.

L. E. P.

THE ELEMENTARY PRINCIPLES OF WIRELESS TELEGRAPHY AND TELEPHONY, by R. D. Bangay, third edition, revised by O. F. Brown, M.A., B.Sc., (Oxon) 267 pages, illustrations, 14 x 21.5 cms. London, Iliffe & Sons, Ltd. Price 10/6.

The purpose of this book is to bring R. D. Bangay's Elementary Principles of Wireless Telegraphy up to date but at the same time preserving as much as possible of the earlier editions. The reviser does this very happily by taking the first twelve chapters, with a few minor changes from earlier editions. In the subsequent chapters today's reader is brought up to date.

In this age of radio when the most modest cottage is equipped to tap the air everyone has a rudimentary interest in this astounding field. The boy or girl who qualifies as an enthusiast will be informed on the theory of electrostatics, the values of E.M.F., transformers, waves, circuits and the tools of reception and projection.

It would have been well if the reviser had included a method for suppressing the nuisance of the neighboring loud-speaker in addition to his description of this tool so useful at a convention. The increasing use of the short-wave, direction-finding by radio and other useful recent developments are treated in the latter chapters of this handy book. Generous illustrations with a clever key, given at the start, to the symbols used in the diagrams of circuits makes it ready reading for the uninitiated as well as a standard work for educational purposes.

H. W. ELKINTON.

AN INTRODUCTION TO THE MATHEMATICS OF MAP PROJECTIONS, by R. K. Melluish, M.A., 144 pages, 14 x 22 cms. Cambridge, University Press, New York, The Macmillan Company. Price $3.00.

This book will serve the serious cartographer as a useful preface to what has become a highly technical art. If some embalmed Egyptian should rub a rotten bandage from the eye and see in this age the development of his early beginnings of map making he would at least see much evidence of the Greek alphabet in the formulæ of the mathematics.

Seriously, the book treats of simple conics, equal area, orthomorphic and perspective projections in chapters II to V inclusive. Miscellaneous Projections, the deformation of projections and finite measurements occupy the next three chapters with a discussion of the best projection for making the map of a given country as the last and IXth chapter. The book throughout is replete with the mathematics essential to this special field of science.

H. W. ELKINTON.

THERMIONIC VACUUM TUBES AND THEIR APPLICATIONS, by E. V. Appleton, M.A., D.Sc., F.R.S., Wheatstone professor of physics, King's College, London. vii–117 pages, illustrations, 11 x 17 cms., cloth. New York, E. P. Dutton & Company, Inc. Price $1.25.

The first specific study of the phenomena upon which the operation of the thermionic tube depends is recorded as due to E. Becquerel in 1853. Many years before that however, in the previous century in fact, the electric conductivity of air in the proximity of heated solids had been noticed. But applicable progress is current history, and appears to have begun with the advent of the Fleming two-electrode rectifier. The growth of the art has been rapid and continued, familiar mostly in its application to wireless apparatus and telephonic transmission. An immense amount of literature on the subject has accumulated in text and monograph form, and it is not a simple matter to find a satisfactorily classified technical account of the principles of operation of those tubes which have proved their value and come into general use.

In this condensed volume, the author meets this requirement in providing a consecutive account of the construction and analysis of the functioning of widely-used types of tubes. He examines their internal action, and develops the equations of their characteristics in remarkable detail considering the small dimensions of the volume. Beginning with a discussion of the diode or two-electrode tube as the parent-type of all thermionic tubes, he passes to the triode or three-electrode which is analyzed at length. This, it will be recognized, is the tube with the deForest grid which has been so widely used in radio receiving sets. Tetrode and pentode tubes having two and three grids are considered in a final chapter. The work is fully documented, and should prove both a certain guide to an efficient study of the subject, and a valuable reference manual to any one professionally interested.

L. E. P.

THE LAST CRUISE OF THE CARNEGIE, by J. Harland Paul, 331 pages, illustrations, 15.5 × 24 cms. Baltimore, The Williams & Wilkins Company, 1932. Price $5.00.

J. Harland Paul, surgeon and observer on the last cruise of the gallant ship Carnegie, tells an interesting tale of the serious and diverting adventures that assailed these sailing men. The Carneige was no fledgling upon this her seventh and last voyage, having served science on journeys I to VI, between the years 1909 and 1921. The purpose of these sailings was to better the survey of the Earth's magnetism and electricity. Incidentally she enjoyed adventure:

"She sailed more than a quarter of a million miles, making some of the longest voyages in history, and traversing all waters between 80° north and 60° south. She had met ice and fogs around Spitzbergen and the South Orkneys, typhoons off Japan, harmattans along African coasts, pamperos near Argentina, hurricanes in the South Seas—and had come through unscathed. She had visited the unfrequented islands, and was without doubt better known the world over than any ship that sailed the seas."

With this send off, the reader will share the pull of release and the tug of joy as the spars bent and the sails bellied to the wind. There is something animate about a ship, especially a sailing ship. The seventh cruise began May the first, 1928, with an uproar of salute from the harbor craft of the Potomac and a genial jockeying by pleasure craft for position of escort. Ten days later this unique, non-magnetic vessel cleared Cape Henry at sunset.

A list of the instruments hardly gives an adequate idea of this vessel furnished so elaborately by the department of Terrestrial Magnetism of the Carnegie Institution at Washington. The autopsy, however, includes a winch for temperature taking and bottom sampling at depths from three to four miles, a plankton pump for the capture (and later the count) of microscopic marine life at given depths, similarly reversing water-sampling bottles and "bottom samplers" for collecting samples of the sea and the underlying land. In addition, instruments for measuring the atmosphere included psychrometers, anemometers, a rain-gauge and an evaporimeter. The sonic depth-finder with oscillator in the keel and the hydrogen ballons to be followed by a theodolite from the deck show the great range of data sought. Add a compact chemical laboratory, an atmospheric-electrical laboratory all in addition to the unusually complete equipment for navigation, and one can sense the serious research carried out on the cruise.

There was a lighter side. Occasionally the Carnegie would touch a port. The amount of space given over to the adventures on land bear witness to a certain relief as well as to the many excursions made to the strange places of the border world. Iceland, the Barbados, old Panama, Balboa, Easter Island, Tahiti, Papeete and Tokio all share in this excitement. The last leg of the voyage suffers a little partly because the Pacific is vast. After recrossing the North Pacific from Yokohama to San Francisco, the Carnegie returned on her last log by way of Hawaii to Samoa. On the 29th of November she was wrecked by explosion in the harbor of Apia. The disaster claimed the life of Tony the cabin-boy and captain James Percy Ault—"whose world-wide scientific service has enriched geophysical knowledge."

HOWARD W. ELKINTON.

THE NATURE OF A GAS, by Leonard B. Loeb, Professor of Physics, University of California, 153 pages, illustrations, 15 × 23 cms. New York, John Wiley & Sons, Inc., 1931. Price $2.50.

This volume marks the first of a series of monographs to be published under the auspices of the Committee on Electrical Insulation of the Division of Engineering and Industrial Research, National Research Council. This Committee is concerned with all things regarding the theory, experimental study, and the performance of dielectric materials as utilized for electrical insulation. Since gases can be and are used as insulators in many instances, it is quite fitting that this monograph "Nature of a Gas" be prepared and published under the Committee's auspices. The Committee also is to be commended for having prevailed upon Professor Loeb to render such a service, he, through his knowledge and ability, being eminently fitted for executing such a task.

The author presents his material in an attractive, logical manner, which renders the book very interesting and instructive while not lacking in those well established hypotheses and mathematical expressions essential for the characterization of a gas. "The Nature of a Gas" consists of three main chapters or sections respectively titled: (*a*) Introduction, (*b*) The Kinetic Picture of a Gas, and (*c*) Ionization Phenomena. In the Introduction, Professor Loeb begins with a description of the elemental divisions of matter, the electron and proton. Starting with these unresolved structural units, the author follows the natural sequence of developments leading to a discussion of the atomic nucleus, its extranuclear structure and finally to molecules, the product of atomic aggregation. The rather complicated implications of extranuclear structure such as the elecronic configurations and their emission spectra are dealt with very effectively.

As might be surmised from its title the section "The Kinetic Picture of a Gas" is occupied with an exposition on the Avogadro Number, Joule's Law, Equipartition of Energy, Mean Free Path, Molecular Velocities and Law of Equipartition of Energy. Under the section "Ionization Phenomena," will be found a general discussion on the types of ionization processes, the probability of such occurring and their probable mechanisms. The book is supplied with a very useful set of appendices which include (*a*) the Radii of Molecules Determined by Various Methods, (*b*) Molecular Velocities, Mean Free Paths and Diameters, (*c*) Conversion Factors and Relations, and (*d*) General Reference and Texts.

T. K. CLEVELAND.

NATIONAL ADVISORY COMMITTEE FOR AERONAUTICS.

Report No. 415. Tests of Nacelle-Propeller Combinations in Various Positions with Reference to Wings. Part I. Thick Wing-N.A.C.A. Cowled Nacelle-Tractor Propeller—by Donald H. Wood, 30 pages, illustrations, 23 × 29 cms. Washington, Government Printing Office, 1932.

This report gives the results obtained in the 20-foot propeller-research tunnel of the National Advisory Committee for Aeronautics on the interference drag and propulsive efficiency of a nacelle-propeller combination located in 21 positions with reference to a thick wing.

The wing had a 5-foot chord, a 15-foot span, and a maximum thickness of 20 per cent. of the chord. The engine was a 4/9-scale model of a Wright J-5 radial air-cooled engine and installed in a nacelle with a cowling of the N.A.C.A. type. The propeller was a 4-foot diameter model of the standard Navy adjustable-pitch metal propeller No. 4412.

The lift, drag, and propulsive efficiency were obtained at several angles of attack for each of the 21 locations. A net efficiency was derived for determining the over-all effectiveness of each nacelle location.

Best results were obtained with the propeller about 25 per cent. of the chord directly ahead of the leading edge. A location immediately above or below the wing near the leading edge was very poor.

Report No. 416. The N. A. C. A. Variable-Density Wind Tunnel, by Eastman N. Jacobs and Ira H. Abbott, 12 pages, illustrations, quarto. Washington, Government Printing Office, 1932. Price five cents.

This report describes the redesigned variable-density wind tunnel of the National Advisory Committee for Aeronautics; it supersedes a previous report that described the original tunnel. The operation of the balance and the method of testing are explained and the method of correcting and presenting airfoil data is described. A summary of the formulas for predicting the characteristics of finite wings from the airfoil section data as they are usually presented is also given.

Report No. 417. Pressure Distribution Tests on a Series of Clark Y Biplane Cellules with Special Reference to Stability, by Richard W. Noyes, 29 pages, illustrations, 23 × 29 cms. Washington, Government Printing Office, 1932.

The pressure distribution data discussed in this report represents the results of part of an investigation conducted by the National Advisory Committee for Aeronautics on the factors affecting the aerodynamic safety of airplanes. The present tests were made on semispan, circular-tipped Clark Y airfoil models mounted in the conventional manner on a separation plane. Pressure readings were made simultaneously at all test orifices at each of 20 angles of attack between $-8°$ and $+90°$.

The results of the tests on each wing arrangement are compared on the bases of maximum normal force coefficient, lateral stability at a low rate of roll, and relative longitudinal stability. Tabular data are also presented giving the center of pressure location of each wing.

The principal conclusions drawn from the results of these tests may be summarized as follows:

1. No biplane arrangement investigated has as high a value of maximum normal force coefficient as the monoplane, although the value for the cellule having 50 per cent. positive stagger and 3° positive decalage (the lower wing at a higher angle of attack than the upper) is only 3 per cent. less.
2. Unstable rolling moments due to a low rate of roll are generally decreased by the use of a gap/chord ratio of less than 1.0, positive stagger alone, or positive stagger and negative decalage.

3. Combined positive stagger and negative decalage show the greatest relative longitudinal stability below the stall.

Report No. 419. Wind-Tunnel Research Comparing Lateral Control Devices, Particularly at High Angles of Attack. I—Ordinary Ailerons on Rectangular Wings, by Fred E. Weick and Carl J. Wenzinger, 26 pages, illustrations, 23 × 29 cms. Washington, Government Printing Office, 1932.

This report is the first of a series in which it is intended to compare the relative merits of all ordinary and some special forms of ailerons and other lateral control devices in regard to their effect on lateral controllability, lateral stability, and airplane performance. The comparisons are based on wind-tunnel test data, all the control devices being fitted to model wings having the same span, area, and airfoil section, and being subjected to the same series of force and rotation tests.

In this particular report the results are given for ordinary ailerons of three different sizes. The medium-sized ailerons, which with equal upward and downward deflection are used as a standard for comparison, had a chord 25 per cent. of the wing chord and a span 40 per cent. of the semispan of the wing. Of the other two sizes, one was long and narrow and the other short and wide. The results are given for five different aileron movements: One with equal up-and-down deflection, one with average and one with extreme differential motion, one with upward deflection only, and one with the ailerons arranged to float with respect to the wing.

The results showed that although the ailerons of medium size with either the equal up-and-down or the commonly used differential motions gave very unsatisfactory control above the stall, satisfactory control was obtained with the short, wide ailerons with upward deflection only, and was closely approached by the same ailerons with extreme differential motion. The short, wide and the medium ailerons with upward deflection only also gave powerful yawing moments which at all angles of attack would aid the rolling, although with small deflections above the stall slight adverse yawing moments occurred. The only ailerons which gave no adverse yawing moments at any deflection or angle of attack were the short, wide ones arranged to float.

Caving Episodes of Underground Exploration, by Ernest A. Baker, M.A., D.Lit., 252 pages, plates, 14 x 23 cms. London, Chapman & Hall, Ltd. Price $6.50.

There are some books that have much more beneath the surface than visible at first. Caving among the caverns of England, Ireland and the continent leads one into a subterranean intellectual pursuit even if it is physically difficult to follow the energetic author down swallets, through waterfalls and up to the chin in the icy waters of underground streams. Ernest A. Baker is no arm-chair speleologist. He professes in this alluring book of underground adventure—inverted mountaineering—to have no interest higher than sport. One escapes from the adit of the last page with a conviction that the party has had a very good time under the genial guidance of one who has climbed to the tops of the Alps, explored the endless caverns of the Mendips as well as publishing "A Guide to the Best Fiction in English (1903)" and several dictionaries. (Cassell's New English Dictionary, 1919, Cassell's French Dictionary, 1920.)

The business of this review, however, is to point out the attractions of the netherworld. The adventures start with Elden Hole, a deep open pot-hole in the limestone hills behind Castleton in the High Peak, a great yawning mouth which appears to have a throat 200 feet long. Such an abyss is easily negotiated by those properly equipped with speleological tackle. The only misfortune in this particular incident was the dislocation of a boulder "about the size of a cottage piano" 50 feet above the author's head. A rock of such size bouncing from side to side of the funnel can do much damage to frail man swinging in a bosun's chair. The chair and the rock enjoyed alternate beats while "an old friend, ensconced safely in the cave at the bottom was heard to ejaculate, slowly 'Oh God! Oh God!' six times."

There is not space sufficient to outline all of the desperate adventures of a hardy cavernist. Forget for the moment the descent into Eastwater on the Mendips of Somerset, on a very rainy day. The peak of the trip, or rather the nadir, led to a very great well 400 feet below the surface, into which poured a torrential waterfall the uproar of which, with the confined and crashing reverberations of an underground chamber, is hard to rid from the ear of the layest of lay readers. Due to the cloud-burst that broke above-ground while the party listened to the blood-curdling din of the Plutonic orchestra a waterfall elected to occupy the entrance of the swallet. (If a waterfall promoted by a down-pour enjoys a diabolic free will.) At all events nine men found the exit barred. They eventually escaped, the cavern, pneumonia and claustrophobia.

There is another compensation to cavernism. Certain favored caves have layers of archeological lore in the debris of their floors. Wookey Hole exceeds according to Mr. Baker, all other places in Great Britain in its record of continuous occupation. Local finds include the relics of Palæolithic man up through the Neolithic, the ages of bronze and early iron, a profusion of Samian and false Samian ware, abundant evidence of Romano-British culture, with subsequent periodic habitation thereafter. "But alas! such serious work is not in my line," writes Mr. Baker.

If one is a serious speleologist one must go to sea, especially on the west coast of Ireland where there is a long range of sea-caves in the limestone coast north of of the cliffs of Moher. Mr. Baker, however, pushes even further to the islands of the Arans where the Firbolgs pursued by the hardier Gaels pushed the defenders from behind the strange fortifications of Dun Aengus into the Atlantic. They could not eliminate all of the weird beauty of this rock ribbed isle.

"The grave of the vanished centuries has for its epitaph the sea-pinks, the sea-companions, the sea-lavender, samphire, and saxifrage, cushioning the limestone, and the maiden-hair fern rooted in the rocks and ruffling its delicate locks in the breeze."

When a cavernist tires of the palaces underground as splendid and vast as those found in the Grotte de Sabart, or as rich in prehistoric art as Niaux or as interesting to the glacial-geologist as the Lambrive which adorn the valley of the Ariege in the Pyrenees, there is always the wild beauty of the upper air to come back to, a precious resource to the arm-chair speleologist.

H. W. ELKINTON.

PUBLICATIONS RECEIVED.

Colloid Chemistry, Theoretical and Applied, volume 4, by selected international contributors, collected and edited by Jerome Alexander, second series of papers on technical applications. 734 pages, illustrations, 16 x 23 cms. New York, Chemical Catalog Company, Inc., 1932. Price $11.50.

Fixed Nitrogen, edited by Harry A. Curtis, American Chemical Society Monograph Series, 513 pages, illustrations, 15.5 x 23.5 cms. New York, The Chemical Catalog Company, Inc., 1932. Price $12.00.

Faraday and his Metallurgical Researches, with special reference to their bearing on the development of alloy steels, by Sir Robert A. Hadfield, Bt. 329 pages, illustrations, plates, tables, 15 x 25.5 cms. Cleveland, The Penton Publishing Co., 1932.

Les Principes de la Mécanique Quantique, par P. A. M. Dirac, traduit par Al. Proca et J. Ullmo, 314 pages, 16 x 24.5 cms., Paris, Les Presses Universitaires de France, 1931.

A Technique for the Analysis of Pitch in Connected Discourse, by C. E. Parmenter and S. N. Trevino, 29 pages, tables, illustrations, 14.6 x 22.6 cms. Reprinted from Archives Néerlandaises de Phonétique Experimentale, volume VII, 1932.

Presidential Address—Section III, including an Account of The Progress of Physics in Canada, by A. Norman Shaw, 14 pages, 16.5 x 25 cms. Toronto, University of Toronto Press, 1932.

Wesen und Wert der Goetheschen Farbenphysik, von Dr. Ernst Barthel, Separatdruck aus Vorstellung und Denken, 14 pages, illustrations, 16 x 24 cms. 1931.

Wesensanalogie und Wesensgegensatz der Farben und Töne, von Dr. Ernst Barthel, Sonderdruck aus "Farbe-Ton-Forschungen," 17 pages, 16 x 23 cms. Hamburg, 1931.

Goethes Farbenlehre—eine Frage der Ehrlichkeit, von Dr. Ernst Barthel, in Der Hochwart, 5 pages, 16.5 x 24 cms. 1932.

Wärmetechnische Beratungsstelle der deutschen Glasindustrie, Frankfurt (Main), Bericht über das elfte Geschäftsjahr, 1931, 7 pages, 21 x 29.6 cms. Frankfurt, 1932.

Bell Telephone Laboratories, Monographs: No. B-652, Zinc Alloys for High and Low Temperature Service, by H. A. Anderson, 19 pages, diagrams, 15.2 x 23 cms. No. B-666, Directional Effects in Continuous Film Processing, by J. Crabtree, 23 pages, illustrations, 15.2 x 23 cms. New York, 1932.

National Advisory Committee for Aeronautics, Technical Notes, No. 418, Compression-Ignition Engine Tests of Several Fuels, by J. A. Spanogle, Langley Memorial Aeronautical Laboratory, 10 pages, diagrams, 19 x 26 cms. Washington, Committee, 1932. No. 420, The Effect of Propellers and Nacelles on the Landing Speeds of Tractor Monoplanes, by Ray Windler, Langley Memorial Aeronautical Laboratory, 12 pages, tables, diagrams, 19 x 26 cms. Washington, Committee, 1932. No. 422, The Aerodynamic Characteristics of a Model Wing having a Split Flap Deflected Downward and Moved to the Rear, by Fred E. Weick and Thomas A. Harris, Langley Memorial Aeronautical Laboratory, 7 pages, tables, diagrams, 19 x 26 cms Washington, Committee 1932.

CURRENT TOPICS.

Forest Fire Hygrometer.—(U. S. Dept. of Agric. Clip Sheet No. 724.) The Forest Service has developed a simple form of hygrometer or moisture-content indicator which forecasts the probability of forest fires. The instument consists of a perforated brass rod a foot long, containing a strip of rattan attached to a hand on a dial. Rattan has a peculiar quality of elongating rapidly when exposed to moisture. The dial hand of the hygrometer, controlled by the lengthwise swelling of the rattan strip and by a tension spring, indicates the percentage of moisture.

Thrust into the litter, or duff, on the ground under trees, this instrument gives accurate readings of the percentage of moisture in much less time than the old method of drying and weighing samples of duff. The probability of fires starting and spreading rapidly is related directly to the moisture content of the forest ground litter so that forest officers equipped with these instruments may be warned of dangerous conditions and take necessary fire prevention precautions. In Northern Idaho, forest workers found that a duff hygrometer reading 25 per cent. moisture content for the duff, means it will scarcely burn at all, while a reading of 10 per cent. or less indicates danger of high inflammability.

T. K. C.

Ice Caps.—(U. S. Dept. of Agric., Clip Sheet No. 724.) Every now and then someone reminds us how much our present living conditions and well-being depends upon a nicely adjusted equilibrium of nature's forces. So delicately stabilized are they, one can not escape the haunting impression these forces closely resemble a host of Damoclean swords any one of which might, without apparent justification, come crashing down. Just the other day we were told that a drop of three degrees in the average temperature of the temperate zone would see a return of the Glacial Period and push the inhabitants some distance south of the Great Lakes. And now Dr. W. J. Humphreys of the Weather Bureau states that if all the ice in Greenland and Antarctica should melt, enough water would be added to the oceans to raise their level 150 feet. Should this happen, our greatest seaports, which include the world's largest and richest cities would be completely inundated.

It hardly is to be supposed the mile-thick ice cap in the frozen north will melt very rapidly although there seems to be no doubt that it is now undergoing a steady recession. Geologists recognize certain times when Greenland did not have much ice and these times may come again. About 1000 A.D. the Danes settled in parts of Greenland that later were glaciated and still later melted again. In the fourteen hundreds there was a marked advance in ice in that area. Over a long period the retreat of the ice is marked. From the 12,000,000 square miles present at the period of last maximum glaciation there now remains only 6,000,000 square miles, most of it being in Antartica.

T. K. C.

Rotenone.—(U. S. Dept. of Agric. Clip Sheet No. 724.) If, as some say, the next great war will be fought between man and insects then it is encouraging news that chemists have at last discovered the structural formula for rotenone, an organic compound recognized as a most powerful insecticide. Utilization of rotenone's poisonous principles had its beginnings in the dawn of history when the Chinese prepared a spray for killing insects from derris, a vine growing wild in Asia and the East Indies. It was also an ingredient of a poison dip for arrows used by the head-hunters of Borneo. In 1902 a Japanese chemist isolated the most potent insecticidal principle of derris and called it rotenone, "Roh-ten" being the Japanese word for the plant and "one" (pronounced "own") a sign that the substance belongs to the chemical group known as ketones.

It was not difficult for the chemist to show that rotenone is composed of atoms of carbon, hydrogen and oxygen present in the proportion of 23 carbon atoms to 22 atoms of hydrogen and 6 atoms of oxygen. However these atoms can be arranged in thousands of different ways and the real task has been the determination of the exact arrangement required to give rotenone. This has been done and the main obstacle to a synthetic production of the substance is now removed. Rotenone is very deadly to insects and at the same time is harmless to human beings if swallowed accidentally. Tests have shown rotenone to be fifteen times more toxic than nicotine to aphids.

T. K. C.

A New Steel Flooring.—From the Mellon Institute of Industrial Research comes an announcement describing a new type of steel flooring called the "Robertson Keystone-Beam Steel Floor." Copies of a report containing detailed practical data and illustrations may be obtained by applying to the Mellon Institute, Pittsburgh, Pa.

It is somewhat difficult to visualize from the submitted description an accurate idea regarding the structure of these flooring members and the report is quoted verbatim: "This flooring consists of slabs, 24 inches wide and up to 12 feet 5 inches in length at present, that are fabricated by preforming two steel sheets and subsequently welding them together in the plane above the neutral axis. A cross-section taken through the width of a completed unit shows four keystone-shaped cells, all connected together near the neutral axis. These four ducts, which constitute each unit, are spaced at six-inch intervals and connect directly, when installed, with the corresponding ducts of the adjacent section of floor slab. This arrangement is described as enabling the utilization of the new floor not only as an efficient load-carrying member, but also as a multiple floor-duct system for handling all types of electrical lines. Such a grouping of parallel cells with six-inch spacing is claimed to permit, at any time, the installation of electrical outlets within a radius of three inches from the exact location required, thus providing permanent electrical flexibility and precluding electrical obsolescence. The keystone slab can be installed with ease and rapidity. It can be bolted, clipped, or welded into place across the structural members, affording a working floor for the various trades.

T. K. C.

Bonded Metal.—In the Mellon Institute progress report for 1931–32 (*Ind. Eng. Chem.*, News Edition, **10**, 97) will be found a brief description of a new protected metal called "Robertson Bonded Metal." This is a laminated metal-felt combination in which felted materials are cemented to steel with heat and pressure, using other metals as the adhesives. The composite laminated material, the outer surfaces of which are suitable felts, is then saturated with any desired saturant chosen with reference to the corrosive condition to which the metal is to be exposed in service. Paint, lacquer and resin films superimposed on the saturated felt give not only added protection but also desired attractiveness of appearance in the finished product. The ductile nature of the metal bond between felt and steel makes it possible to subject this material to forming operations such as shearing, bending, corrugating, rolling and mild drawing without destroying adhesion between felt and steel. Using asbestos felt and a suitable fire proof saturant, this bonded combination gives increased protection over that for bare metal alone. This product also possesses the strength of metal with none of the attendant "ring" and reverberation of bare metal.

T. K. C.

New Carrots for Fur.—(*Ind. Eng. Chem.*, New Edition, **10**, 97.) In the process of felting fur it is necessary first to treat the material with some chemical that will alter its character and make such felting possible. Mercury in nitric acid has traditionally been used for this purpose since the seventeenth century. It was thought such treatment promoted felting by causing the overlapping scales, which compose the surface of the fiber, to stand out and the interlocking of these "barbs" was responsible for the close massing of the fur. The use of mercury is objectionable because of its health hazards while the elimination of the nitric acid would permit a better color, particularly with light fur.

With the object of replacing these two chemicals by others less objectionable and equally as good, a series of researches were carried out at the Mellon Institute. The problem was successfully solved, at the same time proving wrong contemporary theories regarding carroting action. In the course of the investigation it was noticed that dry fur would not felt, but occurred if the carroted fur were kneaded in hot water. The blocking and stretching of formed felt are likewise carried on in the presence of steam. It was therefore determined that the felting quality of fur depended upon its ability to imbibe water, that the property of imbibition was conferred by the carrot, and that fur so wetted became pliant or plastic when heated and relatively rigid on cooling, a behavior analogous to that of gelatin. The barb theory of felting thus gives place to the gelatin theory, the pliant, wet fibers being forced into an interlacing felt, the body of which is maintained by the friction of the fibers, as in any textile fabric.

T. K. C.

Natural Nitrate Deposits in U. S.—Without a doubt, a supply of nitrogen in the form of nitrates is indispensible for our well-being in the present civilized state. Nitrates form an important constituent of fertilizers and nitric acid (nitrate of hydrogen) will transform many an innocuous substance into a powerful explosive. Therefore, it should be of interest that the Geological Survey, after 20 years of search and examination of prospects, has stated that the United States might as well give up the idea of producing commercial nitrate from deposits found within its borders and must depend on by-product or synthetic sources of nitrogen or upon imported nitrates. The report gives general information on nitrates and discusses deposits by counties in 23 states distributed throughout the country. Apparently, of the many nitrate deposits already known in this country not one contains enough nitrate to afford promise of commercial development under any prospective conditions in the country at large, in either the near or the distant future. This report has been published as Geological Survey Bulletin 838 and one of the purposes of its publication is as a warning to the public against certain stock promotion enterprises with claims of nitrate deposits as their basis.

T. K. C.

Thiokol.—Through the mail comes an announcement of a rubber substitute, Thiokol, which is described as an olefinpolysulfide reaction product. Although it is carefully pointed out that this synthetic product contains no rubber, it is interesting to recall that rubber essentially is a polymerized olefin and as such readily combines with sulfur in the process known as vulcanization. Thus, we can expect to find a similarity between the chemical constitutions of Thiokol and vulcanized rubber.

For many uses Thiokol should prove more satisfactory than rubber. The former exhibits unusual resistance to the deteriorating action of gasoline and oils. Because of this Thiokol tubing can be used to convey gasoline from the tank to carburetor in certain makes of automobiles that require a certain length of flexible piping. Unlike rubber, Thiokol compounds do not age or deteriorate in storage. Oxygen does not appear to affect it and even ozone does not cause degradation. No doubt Thiokol will replace rubber in those applications involving conditions specifically detrimental toward the latter.

T. K. C.

The Greater Pennsylvania Council.—A recent report points out that " the methods and the accomplishments of scientific research are being relied upon to an unusual extent by the Greater Pennsylvania Council created by the Pennsylvania Legislature as a state planning and developmental agency, and now at work."

The Council has been instructed by the Legislature to work for " promotion of the economic, social, agricultural, educational, civic and recreational welfare of the Commonwealth of Pennsylvania and its citizens." Dr. Ralph D. Hetzel, President of Pennsylvania State College is Chairman of this Council which consists of 150 Pennsylvania citizens appointed by the Governor. Already the unsparing efforts of this group of leading engineers, technologists and research scientists have begun to bear fruit. A bureau of technical information, which will supply upon request reliable economic, technological and scientific information regarding Pennsylvania, has been established at the Mellon Institute, Pittsburgh.

Free use of this service is invited. Among the important industrial projects may be listed (*a*) a study to find economical methods for purifying certain Pennsylvania clays now unusable because of contaminants; (*b*) a study of the physical, chemical and weathering properties of Pennsylvania stone suitable for architectural and constructional purposes; and (*c*) studies in the economic geography of certain Pennsylvania regions to determine the possibilities for industrial and commercial expansion.

In the field of Agriculture the Council is surveying the lands of one-half the State's area in order to determine their relative fitness for agriculture, reforestation, hunting and fishing, and recreation. In the recreational division a preliminary state-wide survey is being made with a view to encouraging tourist trade through stressing the state's magnificent recreational resources. The General Economics Committee of the Council are studying population trends in the State; the possibilities of attractive and livable homes for persons of small incomes; possibilities for improving business and living conditions; and a study of the industrial possibilities of the area to be benefited by the new Pymatuning Dam in the northwestern part of the state.

T. K. C.

More About Farm Waste.—The U. S. Department of Agriculture announces that by substituting bacteria for chemicals and by adding small quantities of such city waste as sewage to farm waste products, such as cornstalks, straws and hulls, chemists hope to produce building board fiber and fuel and lighting gas by a process which will be commercially feasible. Farm wastes are high in carbohydrates but contain almost no protein. It is the additional nitrogenous city wastes which intensifies bacterial activity, produces a large volume of fuel gas and releases the fiber leaving it available for board making.

T. K. C.

Crying Tin.—Probably few of us have heard that characteristic creaking given out by a bar of tin when bent, such sound being often referred to as the "cry of tin." The generally accepted explanation has been that the "cry" is produced by the grinding of the crystals against one another during the bending of the metal.

In the Apr. 30, 1932 issue of *Nature* will be found a very interesting communication from Bruce Chalmers, University College, London who has seized the opportunity closely observed the above phenomenon while experimenting with single crystal wires. He finds that cadmium as well as tin will give this cry. "When single crystal wires of these metals are stretched, the deformation takes place in two stages, a slip on the glide planes, which constitute in both cases a unique, system, being succeeded by a mechanical twinning on a specified plane." . . . "With single crystal wires no sound is produced during the glide stage of extension, but with both metals twinning is accompanied by the characteristic creaking or tearing sound."

It was found that while the heat evolved during the twinning of cadmium approximated 0.1 calorie per gram, less than one-tenth as much was produced during the process of gliding although the extension was greater in this latter operation. The author points out that a cry can be obtained with zinc which crystallizes in the hexagonal system and twins readily but has not been able to produce this sound in any metal of the cubic system in which twinning does not occur.

T. K. C.

Molybdenum—Today and Tomorrow.—(*Metals and Alloys*, **3**, 98.) J. L. GREGG AND H. W. GILLETT have undertaken to list the more important rôles played by molybdenum in the metallurgical field. The use of molybdenum in forged and rolled steels has been well established for some time. More recently it has proved a valuable addition with either nickel, chromium or manganese for cast steels. In this period of increasing numbers of high-pressure, high-temperature processes it was a fortunate discovery that the presence of a few tenths per cent. of molybdenum greatly increases the resistance of steel to creep at temperatures as high as 1000° F.

The advent of molybdenum's prominence as a valuable alloying element for steel may be traced to its use in the crankshafts of the widely heralded Liberty Motors. Shop experience shows that with the same Brinell hardness, molybdenum steels are easier to machine than other steel alloys. Molybdenum is also credited with increasing wear resistance and has given satisfactory performance when used in shovels, dipper teeth and renewable wearing parts of rock-crushing, grinding and handling machinery. By virtue of its conferred advantages, molybdenum in steel alloys has found a wide variety of applications. Chrom-molybdenum steels are particularly fit for aircraft tubing. Such tubes are easy to weld and a strong tough joint is secured with the usual aircooling. This steel also has been used for barrels of shot-guns which are heat treated after forging to give a tensile strength of 115,000 to 125,000 lbs./in.^{2}. Manufacturers of ball bearings use a steel containing 0.2–0.3 per cent. molybdenum. A railroad is now testing rails containing 0.70

per cent. carbon, 1.60 per cent. manganese and 0.40 per cent. molybdenum which tests indicate such rails will wear 2 to 3 times as long. As stated above, molybdenum greatly increases the resistance of steel to creep of high temperatures and has been used abroad in the socalled high-pressure Loeffler boilers. Cast valves and fittings of the 5 per cent. chromium steel plus molybdenum are considered as particularly adapted to the petroleum industry. Whether the presence of molybdenum in the steel of internal combustion engine valves is beneficial remains in a debatable status. It is known that molybdenum increase the resistance of an 18–8 steel to the action of hot sulfurous acid under pressure and hence may be used as a lining for pulp digestors. In steel castings the effect of molybdenum has been to increase the response to heat treatment and to promote depth hardening. In cast irons, molydbenum seems to markedly strengthen the iron without increasing its machining difficulties. There also are indications that the iron possesses improved wear resisting qualities under conditions of elevated temperatures and hence, such molybdenized cast iron should be particularly suitable for brake drums.

T. K. C.

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